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CONTENTS

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Psychological Bulletin

APPETITE, PALATABILITY AND FEEDING HABIT: A CRITICAL REVIEW[1]

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In 1941 the writer (64) published a critical review of researches upon appetite. Since that date the investigation of problems relating to food acceptance has developed rapidly. Again he wishes to survey experimental studies and to bring his own work into relation with that of others.

Some General References

Experimental methods and results in the study of food habits have been reviewed and a useful bibliography compiled by Dr. Patricia Woodward for the Committee on Food Habits of the National Research Council (78). The principles and theories underlying Richter's work have been generalized in several papers (45, 46, 47, 48). The work of the Food Acceptance Laboratory of the Quartermaster Food and Container Institute at Chicago has been described in a recent paper by Dove (19). From the point of view of food technology the book by Crocker (10) is of general interest. The previous review by the writer (64) contains references to some of the earlier studies and his text (65, p. 125) presents a general statement upon bodily need and appetite. The chapter on bodily needs in Morgan's *Physiological Psychology* (35, p. 437) contains a very clear statement of the facts relating to bodily needs and specific hungers and some useful references.

[1] The present review has been prepared during a sabbatical leave from the University of Illinois which was spent at Harvard University.

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Bodily Needs and Homeostasis

Dietary Requirements

A useful summary of the dietary requirements of laboratory animals can be found in Loosli (**29**). He has listed these requirements under the following headings: protein and amino acids, fat, vitamin A and carotene, vitamin D, vitamin E, thiamine, riboflavin, pantothenic acid, pyridoxine, biotin, niacin, vitamin K, choline, ascorbic acid, other vitamins (not yet isolated), calcium and phosphorus, magnesium, manganese, iron and copper, potassium, sodium and chlorine, cobalt, zinc, and iodine. Water and oxygen are omitted from the list probably because too obvious to mention. A similar summary has been made by McCoy (**30**).

It is of interest to inquire what a nutritionist means when he states that a rat *needs* a particular substance. The answer is fairly simple and might be illustrated many times over from the literature: Every nutritional need is defined in terms of the symptoms or syndrome resulting from a specific deficiency in the diet. One could readily tabulate the various metabolic disturbances and the deficiency diseases which result from withholding one or another of the essential dietary elements. Such a tabulation, however, would be mainly of nutritional rather than psychological interest.

Here is a single illustration: We read that vitamin A is essential for all mammals, that it is important for growth, for normal vision and the maintenance of normal epithelial tissues. Vitamin A deficiency results in keratinization of epithelia, a disturbance of bone growth, sterility and cessation of growth, and, importantly, in night blindness. Vitamin A plays an essential rôle in the metabolism of visual purple. A deficiency, in addition to producing night blindness, may result in a structural breakdown of the retina itself.

In human nutrition the deficiency conditions are commonly described by reference to the symptoms or diseases produced by the want. The vitamins are often described in terms of what they prevent (**20**). Thus vitamin A is antixerophthalmic and antiinfective; vitamin B_1 is antiberiberi and antineuritic; vitamin C is antiscorbutic; vitamin D is antirachitic; vitamin E is antisterility; vitamin B_2 is antipellagric and antidermatitic.

Discussion of such deficiency conditions usually contains a statement of the minimum daily intake of a substance required to prevent the appearance of the symptoms of deficiency. Thus, in discussing the need for thiamine, Loosli states that this need is shown by the deficiency in the metabolism of carbohydrate; there is polyneuritis, cardiac

dysfunction, muscular atrophy and anorexia. One of the studies indicates that 10 micrograms of thiamine per day will cure the polyneuritis and permit satisfactory growth of rats but normal lactation requires a minimum daily intake of 120 micrograms.

Now the vast literature in the field of nutrition is interesting and important for its own sake—but it is not psychology. The nutritionist, being both a physiologist and a biochemist, thinks objectively in terms of growth, reproduction, and a host of developmental abnormalities which result from dietary deficiencies. The developmental abnormalities however, often have very definite behavioral manifestations.

In the past few years, to illustrate, psychological studies have been made upon the effects of amino acid deficiency on the behavior of the rat (**52**), the effect of vitamin B_1 deficiency on the conditioning of eyelid responses in the rat (**6**), convulsive seizures associated with pyridoxine deficiency (**37**), effects of excess glutamic acid on learning in the rat (**32, 77**), spontaneous activity in relation to the diet (**55**).

Animals, in general, exhibit changes in the level of activity when in want of food. Nutritional deprivation is usually shown by a rising level of activity as it is also shown by a decline in weight. Although high running is a typical sign of want, it is not an invariable sign. Wald and Jackson (**59**) have shown that with the deprivation of magnesium in the rat there is a lowering of the level of general activity. After 1 to 2 weeks of magnesium deprivation running declines to a very low level. Magnesium, therefore, is one indispensable component of the diet the lack of which does not stimulate increased activity. The same appears to be true, Wald and Jackson state, for the deprivation in total inorganic ions, and in vitamin A. Apparently, then, if changes in the activity level are ruled out, there remains no single behavioral manifestation of metabolic need, no invariable and dependable sign of need in behavior.

At present there is a fairly large and growing literature upon the psychological manifestations of bodily need. This kind of work, obviously, should be carried on in close collaboration with the nutritionist or biochemist.

Bodily Need and Homeostasis

In his Harvey lecture Richter (**46**) developed the thesis that the behavior of the total organism contributes to the maintenance of a constant internal environment, or homeostasis. The thesis is illustrated by numerous examples:

> If the parathyroid glands are surgically removed, rats develop tetany and die within a few days. When parathyroidectomized rats are given free access to a solution of calcium lactate they ingest large quantities of it, keep themselves free from the symptoms of tetany and survive indefinitely. Along with in-

creased calcium intake there is simultaneous decrease in the intake of phosphorus. If parathyroidectomized rats are forced to ingest a high phosphorus diet the mortality is 100 percent but when given a choice they select a low phosphorus diet and survive indefinitely (**50**).

Again, if the posterior lobe of the hypophysis is removed, there is general dehydration due to loss of an anti-diuretic hormone. The rats compensate for the loss of water by drinking large amounts.

If a toxic substance, such as mercuric chloride, is presented in threshold concentrations of 0.001 to 0.003 percent, the rats reject it and select distilled water even though the total amount of the drug is too small to have any detectable physiological effect.

Adrenalectomized rats ingest quantities of sodium and survive indefinitely. This, Richter believes, is due to chemical changes within the taste buds. When the taste nerves are sectioned adrenalectomized rats no longer increase their intake of sodium and as a consequence they die just as they would have done without access to the sodium chloride. (Work upon adrenalectomy is considered below.)

In general, these and other illustrations show that the behavior of rats is an aid in maintaining a stable *milieu interne*. The drive to maintain internal balance, Richter argues, is one of the most powerful of the biological urges.

Richter's theory is important because, if it is correct, the appetites of laboratory animals can be used in the experimental study of bodily needs. The self-selection method has been employed, in fact, in studies of metabolism as related to endocrine function. It has been used to investigate changes of intake in relation to pregnancy and lactation, age, gustatory function, and other processes. Self-selection feeding has been tried with success upon the human infant.

Nutritive Instincts

Is there a nutritive instinct or group of instincts?

In an interesting discussion Remington (**39**), a nutritionist, has denied that man has any instinct which enables him to select a diet according to his needs. The likes and dislikes of man, he states, are not a dependable guide in the selection of a diet. In a positive and vigorous way Remington points out that the foods which man eats are dependent upon the geographical region in which he resides. The soil and the climate determine what can and cannot be obtained for food. Man of necessity eats what he is able to find in his environment. Further, religious customs and taboos regulate the acceptance of food and determine the manner of its preparation. In modern times Remington continues, economic factors are of basic importance in determining what an individual can and cannot obtain as food. Another determinant is propaganda, in the form of advertising, which builds up attitudes of ac-

ceptance or rejection toward foodstuffs. Thus geographic, economic, and social factors regulate the feeding processes of man. Remington remarks wistfully that up to the present time our knowledge of chemistry, physiology and the science of nutrition has played a relatively minor rôle in directing the eating habits of the human species.

A somewhat different view is that of Dove (**11**) who has emphasized gene-determined individual differences in the ability to select foods wisely. When given the same choice among several foods some chicks appear to select and balance their diet wisely, as indicated by growth and reproduction, and other individuals appear to be less wise in their choice. The same is true of dairy cattle, laboratory rats, and other animals.

Dove (**14**) coined the phrase "aggridant type" to designate the nutritionally superior type of individual within a group. The aggridant type is characterized by superiority in physical form and size, rate of reproduction, longevity, efficiency in the use of foods and consistency in reaction. By preparing dietary mixtures for the group as a whole according to the pattern of foods selected by the aggridant type, Dove (**13**) reports that he was able to produce growth as much as 30 per cent above the standard. He believes (**12**) that the food selections of the aggridant type are useful in providing an optimal environment because these choices have been shown to be nutritionally wise.

Young has consistently stressed the uniformity and consistency of food selections by laboratory rats. Instead of lawlessness and chance the analysis of food acceptance reveals exact quantitative relationships. Consistent individual differences in food acceptance, however, have been observed (**73**) and at the present time no explanation of them is possible.

The work of Blakeslee (**7, 54**) upon the genetics of gustatory thresholds suggests that innate differences in sensory structures, especially in the senses of taste, smell, and touch, may explain why some individuals select food wisely and others do so less wisely.

Dependence of Food Acceptance upon the Organic State

Adrenalectomy and the Appetite for Sodium

As noted above, an increased intake of sodium by adrenalectomized rats has been used by Richter to illustrate the wisdom of the body in the area of food selection. Inasmuch as considerable evidence is available, a more detailed examination of the facts will be profitable.

Increased intake of sodium chloride by adrenalectomized rats has been reported by Richter (**41, 44**), Richter and Eckert (**49**), Mark (**31**),

Clark and Clausen (9), and others. Richter has shown that when adrenalectomized rats are maintained upon a salt-low diet they die but when given opportunity to choose between water and a 3 percent solution of sodium chloride they select the latter, ingesting six or more times the normal amount of sodium chloride, and they survive indefinitely.

Mark (31) reported that the treatment of adrenalectomized rats with desoxycorticosterone acetate (synthetic adrenocortical hormone) decreased their intake of sodium chloride to normal levels. Then Rice and Richter (40) observed that the treatment of normal rats with desoxycorticosterone acetate resulted in an approximately 7-fold increase in the intake of 3 percent sodium chloride. Presumably, they argued, the drug created an increased *need* for salt and this resulted in an increased *appetite*. The polydipsia which the treated animals revealed was regarded as a secondary effect.

Not only do adrenalectomized rats ingest more salt than normals but, according to Richter, their sensitivity to salt is greatly increased. Richter (42) has reported that the salt taste threshold is lowered by adrenalectomy. A group of 20 normal rats revealed an average salt taste threshold of 0.055 percent; a group of 8 adrenalectomized rats had a salt taste threshold of 0.003 percent. The adrenalectomized rats showed a sensitivity to salt offered in amounts which could not possibly have a beneficial physiological effect and, "The results indicate that adrenalectomized rats ingest more salt, not because they learn that salt relieves their deficiency discomforts, but because of chemical changes in the taste mechanisms in the oral cavity, giving rise to an enhanced salt discrimination."

That the taste mechanisms are primarily involved can be shown by sectioning of the taste nerves. After cutting the glossopharyngeal nerve and the lingual branches of the trigeminal (including the chorda tympani) rats no longer were able to recognize the difference between water and salt solutions. When this operation was performed on adrenalectomized rats the section of the lingual nerve resulted in death with a mortality rate of 100 percent (43).

The study of salt ingestion of adrenalectomized rats has been carried forward in two (as yet unpublished) doctoral investigations.

1. Bare (3), at Brown University, confirmed Richter's result that the preferential salt threshold of adrenalectomized rats is lower than that of normal animals. Bare's normal rats revealed a preferential salt taste threshold of about 0.060 percent and his adrenalectomized rats a threshold of about 0.016 percent.

Bare drew an important distinction, however, between the absolute *sensory* threshold and the relative *preferential* threshold. Following the technique of

Pfaffmann (**38**), Bare cut the chorda tympani nerve in the anesthetized rat. The nerve was lifted onto a pair of moist-wick electrodes. Potentials from the cut nerve were amplified and reproduced on a cathode-ray oscilloscope and a loud speaker, making it possible both to see and to hear the impulses. The activity on the face of the oscilloscope was photographed with a movie camera to obtain a permanent record. The rat, placed in a shielded cage with tongue protruded, was stimulated by one drop of stimulus solution at constant temperature. Action currents produced by the stimulation were observed and recorded.

When tested with this technique normal rats responded to solutions of sodium chloride with concentrations of 0.007 to 0.009 percent (average 0.008). Adrenalectomized rats responded to concentrations of 0.005 to 0.017 percent (average 0.010). Although the adrenalectomized rats were somewhat more variable than normals, their absolute gustatory thresholds, as tested with the electrophysiological method, were not significantly different.

The preferential salt taste thresholds of adrenalectomized rats, however, were lower than those of normal animals. The interpretation of this fact is uncertain. It may be, in the light of the findings of Richter and MacLean (**51**) with human subjects, that the preferential threshold corresponds to a concentration at which the solution first tastes salty. Human subjects recognize a *difference* in taste between distilled water and very weak salt solutions at concentrations lower than those which yield a salty taste. Richter believes that rats make a preferential selection of salt solutions at concentrations which first yield a salty taste.

Since rats cannot make introspective reports, it is difficult to determine the concentration which first yields a taste of salt. There may be some other explanation of the lowering of the preferential threshold in adrenalectomized rats.

In any event, Bare is entirely correct in pointing to the difference between sensory and preferential thresholds. If a rat discriminates preferentially between two solutions, we can assume that sensory discrimination between them is possible. If he fails to discriminate preferentially, we are still in the dark concerning the sensory threshold. Under other motivational conditions the rat might make a discrimination.

2. The second doctoral investigation, taking its point of departure from Richter's work, is that of Chaplin (**8, 76**) at the University of Illinois. Chaplin was interested in the preferential selections of adrenalectomized rats among salt solutions of different concentrations.

Chaplin maintained groups of rats upon a complete self-selection diet. In each cafeteria cage he presented either a single 3 percent solution of sodium chloride or a group of 8 solutions of these concentrations: 0.1, 0.2, 0.4, 0.7, 1.5, 3.0, 6.0, 12.0 percent. The average daily intake of each solution was measured both for normal and for adrenalectomized rats.

Chaplin found that there was a marked preference for the solution with a concentration of 0.7 percent which, incidentally, agrees with a result reported by Nelson (**36**). When sodium chloride was obtainable at a concentration of 0.7 percent the rats ingested from 62 to 82 percent of their total sodium chloride from this solution alone. If we assume, with Richter, that the intake at 3 percent concentration indicates bodily needs, then the rats ingested from one

and a half to three and a half times as much salt as they needed when the optimal concentration (approximately 0.7 percent) of solution was available.

Strikingly, however, the optimal concentration for the acceptance of salt was the same for pre-operative and post-operative rats and the optimal concentration was the same as that found in another experiment upon normal animals (**74**). Chaplin had expected that adrenalectomy might lead the animals to prefer saltier solutions but this was found not to be the case.

The experiment differs from threshold studies in that there was at no time a need for salt. The rats, both before and after adrenalectomy, had unlimited access to salt solutions. Under these conditions, incidentally, the adrenalectomized rats were able to survive indefinitely and in seemingly good health and activity.

The diet employed in Chaplin's experiment contained two sources of sodium: sodium chloride and sodium phosphate. The results indicated an inverse relationship between the intake of these two minerals. When sodium chloride was available at the optimal or near-optimal concentration (0.7 percent) the intake of this solution was at a high level and simultaneously the intake of sodium phosphate was at a low level. When sodium chloride was presented at a single 3 percent concentration less sodium chloride was ingested but the intake of sodium phosphate was heightened. The inverse relation between the intake of sodium chloride and sodium phosphate was observed with both normal and adrenalectomized rats. The result recalls the finding of Richter and Eckert (**49**) that there is an appetite for sodium rather than for chlorine.

Chaplin's demonstration that the intake of sodium chloride varies with the concentration of solution, both with normal and adrenalectomized rats, reveals an important distinction. Intake is dependent both upon the organic state (functioning of the adrenal gland) and upon the external characteristics of the food stimulus (concentration of solution). One can argue that rats take what they ***need*** to maintain homeostasis. One can also argue that rats take what they ***like*** (find palatable) regardless of need.

Sickness and Health

References to anorexia and parorexia are widely scattered through the medical literature and, so far as the writer is aware, no one has attempted to bring them together.

An incidental observation is mentioned here because it is of theoretical interest. In one investigation (**72**) an epidemic of bronchopneumonia swept through the colony of rats during the last part of the experiment. Most of the animals were affected but the experimental program was continued. During the epidemic the rate of running upon the apparatus declined; the rats were less active than normal. Further, they became less discriminating between the pairs of test-foods presented for choice. The percentages of preference for all pairs of test-foods dropped toward the 50 percent, or indifference, level. Since tests of preference had been given before the epidemic developed, it was possible to demonstrate that the relative preferences among the test-foods

remained unchanged during the sickness. We are justified in concluding that the bronchopneumonia rendered the animals less active and less discriminating than normals in the selection of food.

Hunger and Thirst

Hull (**24**) and Leeper (**28**) have shown that rats can learn to take one path to food when hungry and another to water when thirsty, discriminating on the basis of their organic state. Young (**70**) confirmed this finding with two methods of testing preferential discrimination.

Hull explained the facts by postulating persistent drive stimuli either from the empty contracting stomach (hunger) or from the tissues of the mouth and throat (thirst). The total pattern of stimuli from the environmental situation and the organic state differed with hunger and thirst. To one total pattern the animal learned, through conditioning, to respond by turning to the right. To the other pattern he learned to respond by turning to the left.

Evidence published by Kendler (**27**), however, presents a difficulty to this internal drive theory of discrimination. Kendler trained rats upon a simple T-maze when they were simultaneously hungry and thirsty. Water was presented in one goal box and food in the other. The experimental procedure was such that all animals would have equal opportunity to explore the contents of both goal boxes. In the critical test series the rats were made either hungry or thirsty but not both hungry and thirsty.

Kendler found that the rats, trained under simultaneous hunger and thirst, were able to respond appropriately during the test trials when they were made either hungry or thirsty. He argued that since there was a single pattern of drive stimuli—that provided by the organic state of simultaneous hunger and thirst—present during training, the hypothesis that rats learn to respond differentially to different patterns of drive stimuli is untenable.

A further argument against Hull's theory is found in the important experiment of Spence and Lippitt (**56**). They trained rats to run a simple Y-maze under thirst motivation. One path of the maze led always to water and the satisfaction of thirst. The other path led to a goal box containing food for half of the rats and nothing at all for the other half. All of the rats could explore the maze but since there was no hunger the rats which found the food did not accept it. There was thus no reinforcement of behavior through contact with food.

When the motivation was changed from thirst to hunger on the first trial with hunger motivation all of the animals continued to go down the

water alley despite the inappropriateness of this behavior. Further, in learning subsequently to go to food when hungry there was no difference between the groups. The animals which had found food in the goal box during exploration (but did not eat it) learned to go to food no faster than the animals which found the goal box empty.

Spence and Lippitt interpret their result to mean that learning depends upon actual reinforcement. "Knowledge," in Tolman's sense, is ineffective apart from reinforcement. Food-seeking habits, therefore, are formed on the basis of actual effects produced by the acceptance of foodstuffs.

Satiation and Deprivation in Food Selection

The above experiments of Hull, Leeper and Young upon the hunger-thirst problem were carried out under the assumption that an internal need creates a specific drive. But another explanation is possible. The hungry rats were also water-satiated and the thirsty rats were also food-satiated. Satiation, whatever its neural mechanism, is revealed by the inhibition of further ingestion. If two patterns of behavior (going to food and going to water) are equally well learned, the inhibition of one pattern through satiation will make the other pattern more likely to appear in a situation which has repeatedly called out both behavioral patterns.

The importance of satiation as inhibitor in food selection is indicated by experiments of Young (**70**) who, in fact, has had to abandon the need-drive hypothesis for one which stresses proprioceptive tensions based upon previous affective reactions (palatability effects). According to the prevailing theory, represented by the above view of Hull, specific food-seeking drives originate in local tissue conditions which provide drive stimuli.

The argument against the local origin of specific food-seeking drives may be summarized as follows:

1. In the experiment of Young and Chaplin (**75**) there was no evidence that protein starvation created a specific urge in the animal to go out and seek the needed protein. The evidence indicated that specific habits of food selection are based directly upon effects of contact with the food via the head receptors. The palatability effects are in some way related to bodily needs.

2. An experiment upon the hunger-thirst balance with different periods of total deprivation demonstrated that the percentage of choices of purina (the preferred nutrient) varied with the number and distribution of reinforcements with apparent disregard for the carefully planned periods of total deprivation (**69**, p. 145). A preference of purina to water developed steadily with practice despite an increasing period of total deprivation from 5 to 144 hours. It was

difficult to reconcile the facts with the view that independent appetites (hunger and thirst) were being balanced one against the other.

3. Control experiments upon hunger and thirst showed that as the period of total deprivation increased from 24 to 72 hours, the rats became increasingly indiscriminate. Position habits dominated behavior more and more rather than the quality of the incentive. In extreme depletion rats were quite indiscriminate in their choice between food and water.

If we abandon the view that contractions of the empty stomach are essential for the hunger drive and that stimulations from the dehydrated mouth and throat are essential for the thirst drive, then we are in a position to look for some other bodily mechanism which can explain differential food-seeking behavior. The nature of this mechanism is indicated by evidence which constitutes another argument against the local-stimulus theory of drive.

Although we have found it difficult to reverse an established preference by creating a need for the non-preferred food, we have repeatedly reversed food preferences by satiating rats upon the preferred food of a pair (**63**). Satiation reduces to zero the acceptability of a food but satiation does not produce an immediate change in food selection as one would expect on the basis of the local-stimulus theory of hunger. Satiation produces a gradual preferential trend toward reversal, but reinforcements are necessary for the preference to reverse. When through controlled pre-feeding a preference has been reversed the cessation of pre-feeding does not produce an immediate return to the original preference but it does produce a preferential trend in that direction.

Apparently some reinforcement is necessary to establish a specific food-seeking drive and the effectiveness of a food as a reinforcing agent varies with the period of deprivation starting from complete satiation as the zero point.

The hypothesis on which we are working is that the inhibition of specific food acceptance through satiation can explain the selection and balancing of a diet in accordance with bodily needs just as adequately as the hypothesis that every need creates its own specific hunger which drives the animal to go forth and seek the food which is needed.

According to the satiation theory of food selection an animal ingests a food until a need is met and perhaps more than met. Some inner mechanism (not clearly understood) puts on the brakes, inhibiting further ingestion at satiation. As a matter of fact, Richter's self-selection method is one which permits a rat to eat to satiation. The changes in intake which Richter observes are changes in the quantity of food required to satiate a rat and to keep him satiated throughout a 24 hour period.

Palatability and the Environmental Determinants of Food Acceptance

In an experiment upon the choice of protein food Aschkenasy-Lelu (2) found that factors other than bodily need are important determinants of food acceptance. There are native individual differences in food acceptance but environmental factors such as the position of food, its familiarity, or novelty, were found to play a part in selection.

The environmental determinants of food acceptance can conveniently be classified under two headings: palatability factors and non-palatability factors. Since our main concern in the present section is with palatability, it would be well at the start to recognize factors other than palatability which are important determinants of food acceptance.

Laboratory experimenters have repeatedly found that the position of a food is an important factor. On the preference apparatus, for example, a rat may persistently take the food located at his right or that at his left with little regard for its quality. If food objects differ in size, as with grains of corn, wheat, or barley, the larger grains are preferred to the smaller. The number of grains (quantity of reward) makes a demonstrable difference in the motivation of the animal. The accessibility of a food is an important factor. Accessibility varies with the kind of container in which a food is presented and with the amount of work which must be done to obtain the food.

Wholly apart from the food itself are environmental conditions which affect the rate of acceptance. One experiment shows that the rate of food acceptance falls gradually as the room temperature rises from 65 to 90 degrees F; above 90 degrees the rate falls rapidly. The rate of water intake makes a sharp rise at temperatures above 90 degrees F.[2] A good many experiments have utilized electric shocks to block food-seeking behavior. If the shock is weak, it may simply retard the rate of food acceptance. More intense shocks may produce an emotional blocking of the feeding reaction. Other environmental factors such as illumination, distraction, humidity, odor, etc., doubtless have a measurable effect upon the process of feeding.

When Richter uses the term *appetite* he is thinking of intra-organic conditions but his index of appetite is intake per rat per day. If the daily intake of calcium increases significantly, the appetite for calcium is said to increase; if the intake falls, the appetite is said to decrease.

[2] Personal communication from Dr. John R. Brobeck, Yale University School of Medicine. For an abstract of Dr. Brobeck's work see: "Food intake as a mechanism of temperature regulation in rats," *Fed. Proc. Amer. Physiol. Soc.*, 1948, 7, 13.

The criterion of intake per rat per day was used by Young (**74**) in an experiment upon the critical concentrations (preferential threshold, optimal concentration, indifference concentration) for solutions of sucrose and sodium chloride. The experiment showed that for sucrose the optimal concentration was approximately at 8.5 percent. For sodium chloride it was at approximately 0.7 percent. When concentrations were above or below the optimal value the rats ingested definitely less of the fluid than at the optimal concentration.

In terms of the theory that intake indicates bodily need one would expect the average daily intake of a substance to be approximately constant under the same set of intra-organic conditions. But actually the quantity of fluid ingested varied markedly with the concentration of the solution. For sucrose and sodium chloride the optimal concentration was one at which there was a maximal intake of water. The maximal intake of the solute occurred at a concentration well above the optimal. The maximal intake of sucrose, in grams, was at a concentration of approximately 18 percent. The maximal intake of sodium chloride, in grams, was at a concentration of about 2 percent.

These variations of intake, depending upon the characteristics of the food stimulus when intra-organic conditions are constant, demonstrate convincingly the factor of palatability. The term *palatability* refers to the acceptability of foodstuffs as determined by the characteristics of the food stimulus. When organic conditions are held constant food ingestion is found to vary with the kind of food presented, with the concentration of solution, with temperature, texture, flavor, and other properties of the food stimulus. Collectively these characteristics of the food stimulus define palatability as distinct from organic appetite.

Relative palatability is revealed not only in terms of intake per rat per day but also by Young's method of immediate choice. In the experiment under consideration tests of preference were given with solutions of sucrose and sodium chloride differing only in concentration. For solutions of sodium chloride the near-optimal concentration of 0.5 percent was preferred to distilled water and both of these fluids were preferred to a 3 percent solution. The method of immediate choice gave results agreeing completely with those obtained when intake was used as a criterion of palatability.

With sucrose solutions, however, the two methods gave different results. Following 24 hours of total deprivation the method of immediate choice revealed that a 50 percent sucrose solution is preferred to a 4 percent sucrose solution and that both of these solutions are preferred to distilled water. In other words, the higher the concentra-

tion the higher the rating in palatability. But with intake as a criterion of palatability the 4 percent solution, being nearest to the optimal concentration, was preferred to the other two fluids.

The explanation of this discrepancy is simple. Richter's method implies that rats ingest a food up to the limit of satiation; the daily intake is a measure of the quantity of a food required to keep a rat satiated throughout a 24 hour period. Less of the 50 percent sucrose solution than of the 4 percent solution is required to keep a rat satiated for a day. Young's method implies deprivation rather than satiation; satiated rats would not run upon the apparatus. Following a day of total deprivation the sweeter solution was preferred to the less sweet. We have no doubt, however, that this preference could be reversed by permitting the rats to pre-feed upon sugar prior to a test (**63**).

Richter has argued that average daily intake indicates bodily need. His results show that rats ingest much more of a substance than the bare minimum required to prevent deficiency symptoms. There is a large margin of safety. But it is also true that a rat accepts what he *likes*.

Richter has used the intake of a 3 percent sodium chloride solution as a measure of the need for sodium. But if the intake at 3 percent is an adequate measure of bodily need, then it can be shown that rats ingest from one and a half to three and a half times as much sodium chloride as they need when the salt is obtainable at the optimal concentration of 0.7 percent. In a brief report Nelson (**36**) has stated that when salt is obtainable at a concentration of 0.8 percent rats ingest much more salt than they need and, moreover, they develop symptoms of excess—enlarged kidneys and other organs, retardation of growth. Incidentally, Nelson's report suggests the interesting possibility of investigating experimentally the symptoms of excess by presenting foods under conditions of optimal palatability.

If rats are given a single salt solution as the only source of water, the result depends upon the concentration. The following tabulation is based upon an experiment by Heller (**23**):

Concentration of sodium chloride	*Result*
0.5–1.0 percent	Normal growth
1.5 "	Subnormal growth, some die
2.0 "	The young die
2.2–2.5 "	Old and young die
3.0 "	Sudden loss in weight, diarrhea, rough hair, death
3.5 "	Young and mature die soon
4.5 "	Die at once

Animals can adapt to salty drinking water to some extent. Rats 4 weeks of age and weighing 50 grams, when placed on 1.5 percent salt solution nearly always died. The ones surviving passed through a stunted period, in time started growing, and in some cases grew normally.

It is an interesting fact that the 3 percent concentration which has been repeatedly employed to indicate need would be lethal if there were no other source of water in the self-selection diet. It is also an interesting fact that the optimal concentration, according to Nelson, may lead to the appearance of symptoms of excess.

In view of the above facts a question can be raised as to whether the concepts of appetite and palatability are both necessary. We tried in a series of experiments to demonstrate a difference between two kinds of preference—one based upon palatability and one upon organic appetite—but we failed to do so (**70**). And now, relying upon Richter's criterion of average daily intake, we are forced to conclude that intake reveals differences in palatability as truly as it reveals differences in appetite and bodily need. An animal accepts what he *likes* as well as what he *needs* and it is an open question as to how far what he *likes* agrees with what he *needs*.

When we are considering the dependence of food acceptance upon intra-organic conditions, such as deprivation, states of satiation, or disturbances of intake related to the endocrine glands, the term *appetite* is appropriate. But when we are considering the dependence of food acceptance upon the food stimulus, its kind or temperature or concentration or texture, the term *palatability* should be employed. Food acceptance is dependent upon two groups of conditions, the one intra-organic and the other environmental.

Experimental Methods for the Analysis of Food Acceptance

The experimental methods which have been employed in the analysis of food acceptance are considered below under three headings: the method of immediate choice, the rate of ingestion, methods dependent upon measurements of intake.

1. *The method of immediate choice.* Two main forms of the method of immediate choice have been employed. In the *foods-apart* method the test-foods are widely separated and in fixed positions as in a T- or Y-maze. The point of choice is beyond the range of the head receptors. In the *foods-together* method the test-foods are presented side by side in accessible containers and their relative positions are alternated from trial to trial (**67, 70**). The latter method has a distinct advantage in that it reveals at once the difference between food preference and position habit.

In the foods-together method the absence of a preference is indicated either

by the consistent acceptance of the food in a fixed position (right or left) or by alternate nibbling of both foods. If the test-foods are exposed for a time longer than that required for choice, the rat may alternately sample the foods. At first we thought that this kind of alternate eating indicated a sampling reaction prior to choice. But we found that when the exposure period was lengthened the alternate eating continued. The pattern is probably best described by Hull's (25) phrase "behavioral oscillation."

The rule in testing preferences by the method of immediate choice is that the test-foods should be exposed no longer than the minimum time required for choice. Since choice is the criterion of preference, the method must require choice and not permit continued feeding.

In the writer's recent work the unit of a preference test has been a pair of successive trials in which both spatial arrangements of the foods are presented. This pair is easily rated as indicating preference for one of the foods or as showing no preference at all.

We have relied upon percentages in studying preferential trends, reversals of preference and the like. These percentages indicate that foods arrange themselves in a truly transitive series such that if A is preferred to B and B to C, then A is preferred to C. When, however, percentages are used to measure distances of separation along the continuum of palatability they are found to be inadequate.

The distance $\overline{AC}$ should equal the sum of the distances $\overline{AB}$ and $\overline{BC}$ but often this relationship does not obtain. Dove (15, 16), relying upon the weight of food ingested as a criterion, encountered the same difficulty with percentages of preference.

One difficulty with the percentage of preference is that it varies with too many conditions. It is known to change with practice and with the familiarity of the apparatus. The percentages vary with the degree of depletion, the state of health, the sensory capacity of the animal, the form of apparatus, as well as with the relative palatability of the test-foods. The percentage of preference needs to be supplemented by other measurements of behavior.

2. *The rate of ingestion.* When test-foods are exposed continuously the best measure of the degree of palatibility is the rate of ingestion. As an animal eats continuously from maximal hunger to satiation the rate of ingestion steadily drops (under some conditions there seems to be an initial warming-up period). Continuous weighing of a food during the period of feeding yields a curve of approach to satiation which shows changes in the rate of acceptance.

The rate of ingestion varies with practice, yielding curves of the type described by Skinner. The rate of ingestion also varies widely from animal to animal.

In one experiment the rate of running upon the preference apparatus was studied in relation to the palatability of the test-foods. The number and the distribution of reinforcements for three foods (sugar, wheat, casein) were held constant and the experiment was designed so that no food had any consistent advantage of priority for the group as a whole. Under these conditions it was found that high palatability is associated with a high rate of running; low palatability with a low rate of running (72). From the rate of running preferences could be correctly predicted.

A criterion commonly employed by nutritionists is the weight of food consumed during the exposure period but there are certain difficulties with weight

consumed as a criterion (62, p. 568). For one thing foods differ in density and a gram of one food, such as salt, is not comparable to a gram of another, such as wheat. Further, the rate of acceptance declines as an animal approaches satiation and the total weight ingested conceals this important fact. Estimates of preference based upon the weight of food ingested vary with the duration of the exposure period.

Relative eating time has the same disadvantage as quantity consumed as a criterion of relative palatability.

3. *Methods dependent upon measurement of intake.* If the test-foods are exposed for a long period of time, such as a 24 hour period, the animal can ingest *ad libitum* up to the limit of satiation and keep himself satiated throughout the period. When the observation is extended for days, weeks or months the daily intake, in grams or cubic centimeters, becomes a measurement of the rate of acceptance. The rate of acceptance is expressed in terms of grams per day or cubic centimeters per day.

In Richter's method of self-selection maintenance the total diet is broken into its components which are presented separately. The subject is free to accept or reject each component and commonly does so up to the limit of satiation.

In some experiments the subject is given no choice. He is placed in a cage containing a single food and his change in weight, activity, and his life span are observed. In terms of survival times the sustaining value of different foods can be determined.

Beebe-Center *et al.* (4) have described a method in which the per diem consumption of a fluid from a single bottle is measured.[3] Solutions of saccharine (or of vanilla-flavored water) and distilled water are presented on alternate days. The difference between the per diem intake of the test-solution and the per diem intake of water is used as a criterion of perference. When the rats are given a single fluid as a source of drinking water the intake of this fluid varies with the concentration of the solution.

In Richter's method for determining preferential thresholds the animal is given a choice between two fluids—distilled water and the test-fluid. At the start distilled water is placed in both bottles. After a few days for adaptation to maintenance conditions a subliminal solution is placed in one of the bottles. The concentration is then gradually increased, day by day, in small steps, until a concentration is reached at which a preference is revealed. Young has modified this method by using both ascending and descending series of concentrations.

[3] Dr. Beebe-Center has demonstrated to the writer a simple and effective technique for measuring per diem intake which eliminates the costly calibrated bottles and the worries concerning uniformity of calibrations. A standard inverted bottle is placed at the top of an individual cage and a straight glass nozzle arranged so that a rat can drink from it directly. A single graduated measuring beaker is used for the entire experiment. A measured quantity of fluid is poured into the bottle prior to exposure. After a 24 hour exposure in the cage the remaining fluid is poured back into the beaker and again measured. The difference is the per diem intake.

The initial measurement could be eliminated by determining once for all the capacity of the filled bottle. Another possibility would be to make the initial and final measurements in terms of weight and converting the series of weight measurements into volume by a single determination of the density of the fluid.

Chaplin (**8, 76**) presented 8 solutions of sodium chloride simultaneously to a group of rats. The animals had free access to the salt solutions as well as to the other components of their maintenance diet. This method was found to be a simple and convenient technique for determining the optimal concentration. It doubtless could be used, with low concentrations, to determine the preferential threshold.

The Motivation of Food-seeking and Food-selecting Behavior

The Local-Stimulus Theory of Internal Drive

The local-stimulus theory of internal drive is probably accepted by psychologists today more widely than any other theory of motivation. Anderson (**1**), for example, in his theoretical paper upon the externalization of drive, assumes that a drive such as hunger is originally dependent for its arousal upon internal conditions of the organism and that through conditioning behavior comes to be controlled increasingly by external stimulation. Hull (**24, 25**), as we pointed out in the above discussion of hunger and thirst, bases his theory of motivation upon primary needs and the local stimuli which they produce in the tissues.

Despite its popularity the local-stimulus theory of internal drive lacks a convincing experimental demonstration.

In a previous review the writer (**64**) referred to experiments by Tsang and Bash which demonstrated that local contractions of the empty stomach are not essential to food-seeking behavior. Tsang surgically removed the main bulk of the stomach in seven rats. He found that after one day of fasting the gastrectomized rats were almost as well motivated as normal animals as evidenced by their efficiency in maze running. Bash surgically isolated the stomach from the central nervous system by destruction of the vagi and splanchnic nerves of rats. With afferent impulses from the empty contracting stomach cut off these animals exhibited a hunger drive which was almost normal as shown by their gnawing through cardboard and learning a maze to reach food. Bash concluded that the hunger drive must operate through a noncerebral reflex mechanism which is probably chemical in nature; food ingestion is a chemoreflexive act.

Similarly, experiments by Bellows and Van Wagenen (**5**), by Robinson and Adolph (**53**), and others, have demonstrated the inadequacy of the view that the thirst drive is reducible to neural stimulation from the dehydrated mouth and throat. Bellows and Van Wagenen found that dogs drank water in normal amounts and diabetic dogs drank excessive amounts when (a) in one group the sense of taste was abolished and the pharynx rendered anesthetic, (b) in another group the excitations mediated by the trigeminal nerves were abolished, and (c) in a

third group the sense of smell was abolished. The urge to drink, therefore, cannot be identified solely with any one of the nervous pathways that were interrupted nor with any single type of excitation which they mediate. Robinson and Adolph have shown that with dogs the signal that initiates the drinking response is a deficit of water relative to the other bodily components. When the body is depleted of water by the amount equal to about 0.5 percent of the body weight, water is ingested. The amount drunk at each draft is accurately proportioned to the body's water deficit as determined by body weight.

In view of these and other experiments the prevailing local-stimulus theories of hunger and thirst are seen to be inadequate. The facts of observation upon which these theories rest, of course, remain as facts but a new interpretation is indicated. The contractions of the empty stomach and the associated conscious hunger pang, the inhibition of saliva and dehydration of the mouth and throat with the painful thirst experience—these should be regarded as *symptoms or manifestations of bodily need* but not as a fully adequate explanation of food-seeking and water-seeking behavior.

The theory of specific hungers lacks a demonstration of internal bodily mechanisms capable of arousing an animal, exciting him, so that he will go out and seek the substance which is needed. In his excellent summary of the facts relating to bodily need Morgan (**35**, p. 437) states that there are at least 11 specific hungers. These are the hungers for: carbohydrate, fat, protein, thiamin, riboflavin, oxygen, salt, phosphorus, sodium, calcium, water. Although the list probably requires revision, it can serve to illustrate the complexity of the psychologist's problems.

The criterion for demonstrating the existence of a specific hunger or appetite is that of independent variability. The demonstration can best be made with purified food substances because natural foods are usually complex from the point of view of nutrition. It can be shown that when a rat is satiated or nearly satiated upon one dietary component he still accepts another. This independent variability leads to the assumption that there are independent specific hungers or appetites. But their number does not necessarily equal the number of known metabolic needs since some needs exist without any consistent manifestations in behavior.

The Nature of Specific Food-seeking Drives and Food-selecting Behavior

In all of the work upon food-seeking and food-selecting behavior it is obvious that the animal *learns* those instrumental acts which lead to

food. He learns to run a maze, to press a Skinner bar, to discriminate between black and white, to dig through sand, to open a door, to shuttle back and forth on a preference apparatus. Outside of the laboratory animals *learn* how and where to obtain food. The bear learns to climb over the mountain to the stream where fish may be caught; cattle learn to roam over the plains to the salt licks; the family cat learns to come to the back door for milk, when a particular creaking sound is heard. Specific food-seeking drives are clearly acquired through a process of learning. There is no mystical insight which leads animals to the food which they need.

The next point is that the speed of food-seeking behavior depends upon practice. For example, a group of 19 rats making 100 runs on our apparatus with casein as an incentive increased their average rate from 1.90 runs per minute for the first 20 runs to 2.71 runs per minute for the last 20 runs (**71**, p. 148). That the rate of behavior changes with practice is a fundamental fact of learning.

A further point of theoretical importance is that the rate of running varies with the degree of palatability of the test-food presented as an incentive. If rats are given a single run per day and the time between release from the starting-box and acceptance of food (the approach time) is measured, the time of approach to a highly acceptable food (sugar) is consistently less than the time of approach to a food of low palatability (casein) (**73**).

Summing up the points made thus far, we may state:

1. That specific patterns of food-seeking behavior are learned.
2. That the rate of food-seeking behavior depends directly upon the amount of practice.
3. That this rate varies directly with the level of palatability of the food which serves as an incentive.

Let us now inquire: What kind of a bodily mechanism can account for the above facts of observation?

One hypothesis is that the rat, when repeatedly placed in a situation which yields food, builds up a specific neuromuscular determination which is capable of regulating the pattern of behavior. There is a specific determination to run to food or to do with the laboratory gadgets whatever is necessary to obtain food. This specific determination is related to what Tolman (**57**, **58**) and others have meant by expectancy. When a rat is repeatedly placed in a piece of laboratory apparatus he builds up an expectancy. Our work shows, further, that a rat builds up a running motive *for a particular kind of food.*

A specific determination to run to food is acquired when an animal

is repeatedly placed in the kind of situation which consistently yields food. As a component part of the determination we assume that there is a change in the tonus of muscles and a change in the pattern of neural excitation which determines the tension of the drive to seek a specific food.

Geier (21) has shown that associated with the expectation of food is a measurable bodily tension. He placed rats for one minute prior to feeding (or non-feeding) in an activity wheel and demonstrated that rats expecting food made more revolutions per minute than rats not expecting food. Underlying his technique is the assumption that when a rat is under tension from some internal drive or from frustration he can and does reduce this tension by running in the activity wheel. This is a way to "work it off." Geier assumed that rats expecting food had a higher level of bodily tension than rats not expecting food.

The present argument carries the hypothesis a step further by assuming that the degree of proprioceptive tension, aroused with the specific determination to run to food, varies directly with the degree of palatability of the food which the animal expects to receive. The determination running-to-sugar, as a fact, is more highly motivating than the determination running-to-casein in the same external situation. The former motivation, therefore, is assumed to come from a higher degree of proprioceptive tension. Since proprioceptive tensions can actually be measured, the hypothesis can be put to experimental test.

The above theory of specific food-seeking drives differs from the local-stimulus theory in one obvious respect: The local-stimulus theory of drive looks for the motivation of behavior in tissue conditions produced by need. Although the nutritionist can describe a wide variety of specific symptoms and syndromes produced by various dietary deprivations, no one has been able to describe diverse bodily mechanisms sufficient in number and complexity to explain the facts. The above theory, in contrast, looks to the proprioceptive mechanisms for immediate motivation in all specific food-seeking drives. These mechanisms are capable of directing the organism to different places with different degrees of proprioceptive tension, or physiological drive strength.

The emphasis in the proprioceptive-tension theory of drive is not upon tissue needs but rather upon the reinforcements from the effects of food ingestion. Immediate palatability effects and deferred organic after-effects furnish reinforcement for the acquisition of selective food-seeking drives.

In one experiment rats were maintained upon a standard adequate diet. In their living cages was an unlimited supply of food and water.

Growth, activity, health were normal throughout. No known nutritional need can be said to have existed. The test-foods, sugar and casein, were supplementary to an adequate diet. Under these conditions distinguishable food-seeking drives, running-to-sugar and running-to-casein, developed under identical conditions. These drives cannot be said to rest upon any bodily need nor upon any known deficiency. The rats were well motivated without need for any known substance (**73**).

Incidentally, psychologists can abandon the view that dietary need is essential for adequate motivation with food. What is necessary is an acceptable reward (sugar) upon which the animals are not already satiated at the time of the experiment. Regular nibbles of sugar can provide adequate motivation to well-nourished rats which are free from metabolic needs.

Feeding Habits

The Basis of Feeding Habits

Although human food acceptance may rest in part upon social pressure, custom and taboo, even upon a sense of duty, these factors do not affect the acquisition of feeding habits by animals.

Habits of feeding rest upon the effects of contact with food. There is, first, the immediate effect produced through the stimulation of the head receptors by the foodstuff and, second, there are remote and deferred after-effects (not clearly understood) which result from the ingestion of food. The term *palatability* refers to the immediate affective reaction (liking or disliking) of an organism which occurs when a food stimulus comes in contact with the head receptors. We know of no general term to cover collectively the various comforts and discomforts, reliefs, and cramps, satisfactions and dissatisfactions, which commonly follow the ingestion of certain foods.

Palatability and the law of effect. According to the law of effect, the satisfaction derived from food acceptance reinforces the patterns of behavior which produce that satisfaction.

Two investigations by Young (**72**, **73**) bear upon the law of effect. Both investigations justify a distinction between the rate of running to food and the rate of learning. The experiments show that at all stages of practice rats *run* faster in approaching a highly palatable food (sugar) than in approaching a food of low palatability (casein). The difference in the time required to approach and accept these foods indicates a motivational difference, a difference in the strength of physiological drive. But if the data are analyzed so that differences in the rate of running are equalized or balanced, the rate of *learning* the habit of

running-to-sugar is the same as the rate of *learning* the habit of running-to-casein. The rate of acquisition depends upon the frequency and distribution of reinforcements rather upon than the quality of the reward. If the frequency and distribution of reinforcements are held constant, then rats do not learn faster for one food incentive than for another, although they undoubtedly run faster to accept a good food than to accept a bad food at every stage of practice.

The data illustrate the distinction between performance and learning. Performance depends upon many factors (practice, motivation, state of health, external temperature, etc.). Learning depends upon practice which is one of these factors.

If learning is defined as acquisition dependent upon practice alone, then the law of effect is not a law of learning, since the rate of acquisition is independent of effect. But this definition of learning, perhaps, is too narrow. Determinations to run (motives) are acquired as truly as motor skills. The determination to run to sugar or to run to casein depends directly upon the effect of contact with these foods.

Therefore, we suggest a broader definition of learning which includes the acquisition of motives (specific food-seeking drives and food expectancies) as well as the acquisition of motor skills (activities which are instrumental in obtaining food). A broader definition would assert that learning is the acquisition of behavior dependent upon (a) practice or (b) effect. Further research on this problem is clearly needed.

Intra-organic after-effects and learning. The experiments of Harris *et al.* (22) point to the importance of intra-organic effects in habit formation. The nature of these after-effects is not clear but they are related in some way to increased rate of growth, more rapid heart beat, change in alimentary tonus, and relief from the symptoms of dietary deficiency.

Harris *et al.* believe that the behavior of a rat in selecting food is due to his experience of some beneficial effect produced by ingesting a particular food. If rats are depleted of vitamin B, they select a food containing the vitamin and begin to gain in weight. When given a choice between diets containing different concentrations of the vitamin they select the food with the higher vitamin content. Rats are able to select the vitamin-containing food whether it is presented in yeast, wheat-germ oil, or marmite, even if a barely sufficient quantity is present in the food.

When several foods (6 to 10) are presented simultaneously some of the vitamin-depleted rats succeed in selecting the food containing the vitamin but most of them, according to Harris *et al.*, fail to make the selection. Two or three days of "education," however, lead them to

select the adequate diet. "Education" consists of letting a rat feed on the vitamin-containing diet until he can experience its beneficial after-effects, *i.e.*, until he can recover from the discomfort of avitaminosis and resume normal growth. In "education" the rats are trained with food of a particular flavor. If the food is associated with recovery, they then continue to select the food of that flavor even if the vitamin is later withdrawn.

In one experiment 4 rats were depleted of vitamin B. They lost weight. Then they were trained to accept a bovril-flavored diet containing an adequate amount of the vitamin. When offered a choice among basal diets of four flavors (basal diet alone, this flavored with lard or with cocoa or with bovril) the animals continued to select the bovril-flavored diet. The vitamin was then transferred to the cocoa-flavored diet but the rats continued to select the bovril-flavored food. They were thus deceived. Through "re-education" on the cocoa-flavored diet, however, they learned to accept it and they gained in weight. This time they continued to select the cocoa-flavored diet when given a choice.

These investigators conclude that the ability of the vitamin-depleted rat to discriminate between diets containing the vitamin and diets deficient in it depends upon an association between some distinctive character of the diet (smell, taste, feel, appearance) and prompt experience of some beneficial after-effect. If the effect is not associated with the food, a rat may fail to make a wise choice.

In an experiment by Young (**70**, pp. 1–17) a single vitamin solution containing thiamin, riboflavin, pyridoxine, nicotinic acid, and pantothenate acid, in the concentrations recommended by our adviser on nutrition, was presented to a group of rats. For some unknown reason the vitamin solution was rejected and the rats began to lose weight. The rats were then forced to drink the vitamin solution by removing for two days all other sources of water. The animals drained the bottles dry, obtaining therefrom on the average 12 c.c. of the vitamin solution per rat per day. After this "education" the original diet was restored. The rats, strikingly, continued to ingest the vitamin solution in sufficient quantity and they gained in weight.

The moral is that the nutritionist, basing his advice upon known metabolic needs, lacked dependable information concerning the palatability of vitamin solutions presented singly and in combination. Hence he could not prepare a compound solution which the rats would accept. But it should be relatively easy, with the behavorial techniques now available, to prepare a solution which rats would accept. The practical art of feeding must rest upon sound psychological analysis of food acceptance.

How Feeding Habits Can and Cannot Be Changed

The committee on Food Habits of the National Research Council has been concerned with the problem of changing food habits (**78, 79**). Dr. Margaret Mead (**33, 34**), Secretary of the Committee, and other members, have placed a good deal of emphasis upon social and cultural factors in the causation and change of food habits. Although this emphasis is clearly justified, the review of social and cultural factors is beyond the scope of the present paper.

Our approach is from the point of view of experimental psychology and we ask: What does recent research show about the ways in which feeding habits can and cannot be changed? The question will be considered under four main headings:

1. *Feeding habits stabilize patterns of acceptance.* Rats thoroughly habituated to the selection of sugar in a choice between sugar and casein continued to choose sugar despite a protein starvation of 32 days with marked signs of dietary deficiency. When tested with a different apparatus, however, and with a different technique they at once developed a preference for casein in agreement with their metabolic needs (**75**).

Our first interpretation of this result was that two kinds of food preference existed simultaneously, one based upon immediate stimulation of the head receptors (palatability preference) and the other upon organic need (appetitive preference) (**66**). Subsequent control experiments, however, showed that this interpretation was not correct. We have been unable to demonstrate a difference between palatability preference and appetitive preference (**70**).

The consistent selection of sugar in preference to casein demonstrated the persistence of a well-established feeding habit which happened to be opposed to a bodily need. The consistent selection of sugar was a manifestation of habit alone wholly apart from need and probably apart from palatability (assuming the palatability of casein to have changed with need for this food).

The general principle in this and similar experiments is that established feeding habits tend to persist, to stabilize feeding behavior, regardless of bodily needs but new habits tend to form in agreement with bodily needs (**69**). Change of apparatus forces the animal to become exploratory and more discriminating and the change of environment is favorable to the development of feeding habits which agree with bodily needs (**71**).

2. *The effect of training varies with palatability.* A common human method of attempting to change feeding habits is to force a child to accept a food he dislikes. Is this method effective with animals?

In one experiment two groups of rats, maintained upon the same adequate diet, were trained to run the preference apparatus. One group ran for nibbles of sugar and the other for nibbles of wheat. After training both foods were presented together in a test of preference. The rats trained to run for sugar selected sugar when given a choice although the preference was not stable and some animals changed to wheat. The rats trained to run for wheat continued to select wheat. In other words, the rats tended to select the food upon which they had been trained (**67**).

Since sugar and wheat are nearly equal in palatability, it was decided to see

whether training on a food of low palatability could make it more acceptable than sugar. Rats, maintained upon a complete self-selection diet, were given a choice between sugar and casein following 24 hours of total deprivation. They consistently preferred sugar. They were then given repeated runs for casein alone. Practice up to 1000 runs per rat did not change the initial preference. On the contrary, the preference for sugar increased from about the 60 percent level to a consistent 100 percent preference. There was no evidence that forcing the rats to accept an unpalatable food raised the level of its acceptability. Incidentally, practice in running for casein resulted in an increase in the rate of running. In a series of 900 runs the average rate for the first 20 trials was 0.83 runs per minute and for the last 20 trials 3.98 runs per minute (**71**).

The two experiments appear to yield contradictory results but it should be remembered that the conditions were different. Sugar and wheat are highly palatable and nearly equal in acceptability to rats. Casein is a food of low palatability. Keeping these differences in mind, we can summarize the two results in a single generalization: If two foods are nearly equal in palatability, the repeated acceptance of one develops a feeding habit which is temporarily effective as a determiner of choice; but if two foods differ widely in palatability, training in accepting the non-preferred food is ineffective in changing the preference.

3. *A preferential habit can be changed by satiating the animal upon the preferred food.* If a preferential habit has been well established, it can be reversed by pre-feeding the preferred food immediately before a test of preference. The reversal does not appear at once but it comes gradually with repeated runs. After a preference has been reversed there is a gradual trend toward the original preference when pre-feeding is discontinued (**63**).

In one experiment it was found that rats preferred sugar to casein following 24 hours of total deprivation. When the rats were deprived of casein and satiated upon sugar, in the early stages of training, the preference reversed; the rats selected casein in preference to sugar. With continuous casein deprivation of 21 to 25 days the sugar was removed from the diet giving a simultaneous sugar deprivation of 1 to 5 days. Although the rats were weak and inactive, due to the prolonged deprivation, they returned to their original sugar preference (**70**).

There is no doubt that preferential food habits can be controlled by varying the organic state.

4. *A feeding habit can be changed by changing the palatability of the foods.* Since the rate of acceptance of salt and sugar varies with the concentration of solution, an animal can be made to accept more or less of these substances in his total diet by changing the concentration. When the intra-organic conditions are held constant heightened intake is a mark of heightened palatability (**74**).

Affective Psychology and the Science of Nutrition

In a critical review of current approaches to affectivity Hunt (**26**) made two pertinent statements:

> It would seem fairly safe to assume that most psychologists today would agree that the concepts, pleasantness and unpleasantness, as used in psychology tacitly refer to general attitudes of acceptance and rejection and that the field of affectivity covers the investigation of these attitudes in their development

and operation. . . . A brief summary would run something like this: The organism may either accept or reject a stimulus. This acceptance or rejection is carried out through appropriate bodily adjustments. These reactions are said to constitute the affective response and are assumed to be a functional entity of some kind.

There are a good many factors which regulate the relative acceptance or rejection of foodstuffs but the investigation of these factors falls squarely within affective psychology.

The nutritionist has a different point of view. The writer recently heard a distinguished biologist describe a method of feeding laboratory mice which was considered ideal from the nutritionist's point of view. A mouse was trained to swallow a food pellet introduced directly into the mouth and throat. The constituents of the pellet could be precisely controlled. No more worries about palatability and habits of feeding!

Dove (**17**) has attempted to combine the nutritional and psychological points of view. He starts from the *appetite level* of consumption which is the actual level of intake for an individual or a group. It is not enough to limit consideration to the nutritional values of foods in the practical art of feeding men and animals. Dove (**18**) has stated the proposition clearly: "*Each food must be evaluated not by what is possesses but by what it gives to the consumer;* and it gives to the consumer in gross value its percent value per unit weight times the weight of food accepted." Food acceptance is regulated by psychological factors.

In the Quartermaster Food and Container Institute for the Armed Forces, in Chicago, Dove (**19**) has been conducting a series of investigations upon problems of food acceptance. The research is comprehensive in scope, embracing analysis of regional differences in food habits due to geographic, cultural, economic, and other determinants, as well as psychological research upon the problems of feeding.

In the war which has just closed the soldier-consumer was provided with a ration calculated by nutritionists to be complete in supplying the necessary minerals, vitamins, proteins, fats, and the requisite calories for energy expenditure during heavy work. But frequently soldiers refused to eat some of the items in the ration and threw their food away! As Dove said has: "when war comes it is easier to dress men alike, even though they come from different regions, with different social, economic, and cultural and racial origins than it is to feed them alike."

Conclusions

1. Dietary need is a nutritional concept. When a component of the diet which is required for normal growth, reproduction, activity, or for survival itself, is removed a pattern of deficiency symptoms appears.

Deficiency symptoms are not drives. No one has been able to demonstrate that for each specific deficiency there exists a specific form of food-seeking or food-selecting behavior.

There are, however, behavioral manifestations of depletion as well as structural changes. A specific deprivation may change the level of activity, the time and error scores in maze learning, the liability to fits, or the functional capacity of the sense organs.

2. Habits of seeking particular foods appear to rest directly upon the effects of ingesting these foods. The term *palatability* implies an affective reaction to foodstuffs which stimulate the head receptors. There are also delayed and remote after-effects of food ingestion which, under some circumstances, may be the basis of dietary habits.

3. An established feeding habit may persist regardless of bodily needs. On the other hand, new habits tend to form which will meet bodily needs.

4. Among the organic determinants of food acceptance the conditions of satiation and deprivation are of prime importance. As an animal steadily ingests a food, approaching satiation as the limit, the rate of acceptance declines. If satiation is the zero point of food acceptance, the acceptability of a food increases with the period of deprivation starting from the zero point of satiation.

As the period of deprivation increases rats become less and less discriminating among foods and more and more determined by position habits.

In addition to satiation and deprivation there are other organic conditions which regulate food acceptance such as the balance of the endocrine glands, age, pregnancy and lactation, sickness, etc.

5. The environmental determinants of food acceptance can be classified as palatability factors and non-palatability factors. Palatability factors are characteristics of the food itself such as the kind of food, concentration of solution, temperature of the food, texture, etc. The experimental study of palatability is of great importance in the practical art of feeding men and animals.

Closely related to palatability in the regulation of food acceptance are such determining conditions as size of food object, quantity of food, position of food, degree of contamination, kind of container, laboratory apparatus through which food is obtained, etc. Environmental factors not directly related to palatability but modifying the feeding process are temperature of the surroundings, distractions, emotionally disturbing shocks and noises.

6. Strength of drive, as measured by the time required to approach

and accept a food, is positively correlated with the degree of palatability of the incentive. Animals run faster in approaching a highly palatable food than in approaching one of low palatability.

The rate of habit growth, however, is not dependent upon the degree of palatability of the incentive. Learning depends upon the frequency and distribution of reinforcements.

7. The conditions which regulate food acceptance are numerous and they are complexly interrelated. There are three main groups of conditions, indicated by the title of this paper, which must be controlled by laboratory workers: conditions within the organism (appetitive conditions); conditions within the nutritive environment (palatability and non-palatability determinants); conditions within the previous behavior of an organism (feeding habits).

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AN EVALUATION OF THE STUDY BY BERNARDINE G. SCHMIDT ENTITLED: "CHANGES IN PERSONAL, SOCIAL, AND INTELLECTUAL BEHAVIOR OF CHILDREN ORIGINALLY CLASSIFIED AS FEEBLEMINDED"[1]

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INTRODUCTION

Since the time of Itard many individuals have cared for, trained, and educated children with low intelligence. Throughout the United States many children have been, and are being, trained and educated in institutions and in special classes in numerous school systems. Many individuals have written about the improvement and adjustment of these children to our society in terms of vocational and social adjustment at the level of their mental capacities.

A recent report by Dr. Bernardine G. Schmidt[2] has presented data which show an improvement in intellectual, social, and personal characteristics of feebleminded children far beyond that which has ever been presented by any other writer. In brief, the monograph describes an eight-year study, which may be abstracted as follows:

1. 254 children were initially examined by the psychologists of the Bureau of Child Study of the Chicago Public Schools. According to the monograph these children were classified as feebleminded and were referred to special classes in the public schools of the city of Chicago.
2. The initial IQ's of these children as determined by the Bureau of Child Study psychologists at the time of referral averaged 52.1, as shown in Table I.
3. At the completion of three years of training the IQ's had risen to 71.6. (See Table I.)
4. At the completion of five years of post-school experiences the IQ's of the children had increased to an average of 89.3. (See Table I.)
5. The social maturity scores showed even greater improvement than did the IQ's.
6. 27% of these "feebleminded" children completed a four-year high-school course. 5.1% continued post-high-school training (p. 117).
7. The employment record and the social adjustment of these children showed similar results.

[1] SCHMIDT, BERNARDINE G. Changes in personal, social, and intellectual behavior of children originally classified as feebleminded. *Psychol. Monogr.*, 1946, **60**, No. 5, 1–144.

[2] The writer wishes to express his acknowledgements and appreciation to the *Psychological Monographs* and to the American Psychological Association for permission to quote from the text and tabular data of the monograph evaluated in this study.

TABLE I*

MEAN SCORES FOR EACH STANDARD MEASURE (1) AT ENTRANCE, (2) AT CLOSE OF IN-SCHOOL PERIOD, AND (3) AT THE CLOSE OF POST-SCHOOL PERIOD

Group	*Stanford-Binet*			*Vineland*			*Bernreuter*		
	1	2	3	1	2	3	1	2	3
I	50.5	71.6	89.3	60.3	87.4	107.2	32.0	−83.6	−123.0
II	54.4	72.0	83.8	59.1	94.1	84.8	76.0	33.2	− 12.6
III	47.7	76.2	83.0	64.7	89.3	83.8	52.0	2.8	− 12.5
Total	52.1	71.6	89.3	59.4	91.8	107.2	52.0	−27.2	−123.0

* From Schmidt, B., *op. cit.*, Table 72, p. 87. The data for the Willoughby Test and the Detroit Test have been deleted. These personality tests show similar improvement.

Since these results are in sharp contrast to current professional opinion, it is desirable that these data be given all possible substantiation. Many lay individuals and some professional people have uncritically accepted the results, while others have uncritically rejected them as impossible and fantastic. The editor of the *Psychological Monographs* in publishing the monograph included a rather unusual prefatory statement. Because of its nature this prefatory statement is reproduced here:

PREFATORY STATEMENT

In view of intense and critical advance interest in the following study which has arisen in consequence of a preliminary publication of a summary (cf. *School and Society* 1945, **62**, 409–412) it seems appropriate to call the attention of readers to a somewhat unusual situation in respect to scientific policy in publication. Where a piece of research produces results which appear to be in sharp contrast to conventional professional opinion, and especially if there is an element of controversy, a scientist might be expected to withhold publication pending a repetition of the research, preferably under independent auspices. In the present case, however, although several such repetitions of this investigation are, we understand, at the point of being initiated, the unusual scope of the experiment, involving some eight years for the collection of the data plus several more for preliminary planning and the subsequent analyses, would entail a delay of many years if publication were to be held up. Because of the desirability of making available the full report of the study* for those who may wish to repeat various aspects of it for verification or extension, the undersigned believe it desirable to publish it at this time.

Signed,
Bernardine G. Schmidt (author)
F. C. Rosecrance (member of examining committee)
Robert H. Seashore (member of examining committee)
John F. Dashiell (editor)

* The scope of the doctoral thesis in the School of Education, Northwestern University, upon which this publication is largely based, was limited to the educational, social, and vocational development of the subjects, but did not at that time include the data on development in intelligence test scores which were a part of the total directed study and are included in the present publication. (*Ibid.*, p. iii.)

In reviewing a controversial issue, a researcher should be as impartial and objective as possible. It is rather difficult to obtain an impartial investigator since most individuals interested in investigating such an issue have a point of view or point of reference which may influence the outcome of the investigation. If an investigator is not unbiased, then the direction of his prejudice should be made known. For that reason the bias of the present writer will be exposed by referring to certain statements made in writing several years ago. The writer[3] stated:

> Although recent evidence on the influence of environment on the IQ is taken by some to indicate that intelligence is the product of training, there are others who cling to the view that intelligence is inherited. The answer is probably somewhere between the two views. It is probable that inheritance fixes the limits of intelligence but there is a large range within which the environment can raise or lower the IQ.

The data presented by Dr. Schmidt appear to be quite conclusive and if these data are accurate, she has demonstrated large improvements in intelligence test scores and in social and personal adjustments of "feebleminded children."

The prefatory statement quoted earlier implies that the study reported by Dr. Schmidt should be verified by repeating the study over a period of eight years. The present writer disagrees with this statement for the following reasons:

1. The techniques described by Dr. Schmidt are not entirely new, but rather are standard practice in any first class training program for the mentally handicapped. This study has been repeated many times without the results shown by Dr. Schmidt's data. There are many fine teachers of the mentally retarded in every large city of the United States who employ similar methods of training to those described by Dr. Schmidt.
2. If another person repeated the study over a period of eight years and obtained negative results, the results in themselves would still not disprove the data presented by Dr. Schmidt. It could always be stated that the teaching used by a new investigator was not as effective as Dr. Schmidt's teaching methods. Actually the only person who can repeat the study for verification purposes is Dr. Schmidt herself.

[3] KIRK, SAMUEL A. *Teaching reading to slow learning children*, Houghton Mifflin Co., 1940, p. 46.

3. A more expedient and possibly more adequate suggestion is the desirability of an outside researcher rechecking the original data, as they appear to be the only portion of the study that deviates from usual expectation.

Procedure of Evaluation

In attempting to find some method of checking the original data two possible approaches appeared to be feasible:

1. Examine the data presented by Dr. Schmidt to determine the agreement of that data with the statistical data for special classes in the city of Chicago.
2. Check the initial IQ data on all of the children in Dr. Schmidt's study by examining the records in the files of the Bureau of Child Study, since according to Dr. Schmidt the children were all tested by the Bureau of Child Study.[4]

It was believed that an evaluation by the two methods listed above would throw some light on the authenticity or accuracy of the initial data presented by Dr. Schmidt.

Results

The Correspondence of the Initial Data with the Statistics for Chicago Special Classes as a Whole.

Dr. Schmidt asserts that she studied 254 children who were placed in special classes in five centers in the City of Chicago. Her report of initial test scores from these centers is reproduced below:

TABLE II*

Range, Medians, Means, and Sigmas of Intelligence Quotients for each Contributing Center

Center	*N*	*Range*	*Median*	*Mean*	*Sigma*
A	110	27–69	50.64	49.58	10.4
B	91	35–69	55.46	53.47	8.6
C	53	27–69	55.20	55.6	10.2
D	31	55–69	63.20	61.3	2.3
E	37	50–69	62.10	62.5	2.1

* From Schmidt, B. *op. cit.*, Table I, p. 9.

The initial test data relating to IQ's, as shown in Table II reveal rather unusual distributions. For example, in Center *A* the median IQ

[4] Dr. Schmidt asserts (p. 7–8) "They had been placed in these special centers on the recommendation of the Child Study Bureau of the city schools, following the administration of individual intelligence tests by the psychologists of the Bureau."

for 110 children was 50.64. This means that approximately 50 percent of the children had IQ's of 50 or less. These data do not appear to be in harmony with the usual composition of special classes in public school systems. Most school systems admit children to special classes with IQ's ranging from about 50–75, with an average IQ in the 60's. (The Centers *D* and *E* in Table II are in addition to the 254 children in centers *A*, *B*, and *C*, and served as a control group for the study.)

To determine the practice in the city of Chicago itself, statistics of special classes in the city were perused. The following table shows the type of child referred to special classes in the city of Chicago, for the years 1937 to 1940.

TABLE III[8]

INTELLIGENCE QUOTIENTS OF CHILDREN RECOMMENDED FOR UNGRADED DIVISION

Intelligence Quotients	*Calendar Year 1937*		*Calendar Year 1938*		*Calendar Year 1939*		*Calendar Year 1940*	
	Number	*Percent*	*Number*	*Percent*	*Number*	*Percent*	*Number*	*Percent*
20–29	0	—	0	—	1	.1	0	—
30–39	1	.1	2	.1	4	.2	5	.3
40–49	33	1.7	40	1.9	48	2.7	45	2.8
50–59	219	11.0	235	11.0	223	12.5	199	12.5
60–69	587	29.4	692	32.4	638	35.7	518	32.5
70–79	1,010	50.7	1,048	49.1	813	45.5	746	46.8
*80 or above	84	4.2	76	3.6	24	1.3	64	4.0
Not Determined	60	3.0	39	1.8	35	2.0	18	1.1
	1,994	100.1	2,132	99.9	1,786	100.0	1,595	100.0
Mean I.Q.	69.1		68.7		68.0		68.1	

* Children with IQ's of 80 or above are not recommended for placement in the Ungraded Division except in unusual situations.

It should be noted from Table III that the statistics on the distribution of IQ's in the special classes do not correspond with those of Dr. Schmidt (Table II). The following differences are found:

1. Dr. Schmidt's table (Table II) shows IQ's ranging from 27–69. *Not one case initially had an IQ over 69.* The Chicago Schools' statistics from 1937–40

[8] Table reproduced from page XIV, *Bureau of Child Study and the Chicago Adjustment Service Plan.* 1940–1941, Board of Education, City of Chicago. (Out of print but available in large city and University libraries.)

(Table III) showed that approximately 50% of the children in special classes have IQ's between 70 and 79.

2. The mean IQ for Dr. Schmidt's children is in the low 50's. The mean IQ for the Chicago special centers as a whole is consistently 68 or 69.

3. It is statistically almost inconceivable that the 254 children assigned to Dr. Schmidt could have differed so widely from the general population from which they were drawn. Furthermore no explanation was made in the study by Dr. Schmidt indicating that the groups of children which she studied differed markedly from all other special class groups in the city of Chicago.

Authenticity of Initial IQ Scores

One method of evaluating the data is to obtain the names of all the children in the study, and to look up the initial IQ's of the children in the files of the Bureau of Child Study. The present writer corresponded with Dr. Schmidt requesting that the names of the children in her study be submitted to him for the purpose of checking the original data. Dr. Schmidt did not reply to the first letter for a period of two months. After a second letter, Dr. Schmidt wrote as follows:

> I am sorry, but I can not be willing to furnish to you the identities of the children in the Chicago study, for the purposes you state.

The writer made a visit to the city of Chicago,[6] examined the personnel files on Dr. Schmidt, visited the school in which she had worked for five years, and studied the records of the Bureau of Child Study. The following information and data were obtained.

1. The personnel records showed that Miss Bernardine Schmidt was first employed as a teacher at the Lafayette school in the Chicago Public School System in September, 1936. She was qualified to teach mentally handicapped children since she had just obtained her certificate to teach subnormal children in June, 1936, from the Chicago Teachers College. As one requirement for this certificate she had done cadet teaching at the Lafayette school during the second semester of 1935–36. Miss Schmidt had previously obtained a three year elementary teaching certificate from the same Teachers College.

2. A visit to the Lafayette school disclosed Miss Schmidt's "Teacher's Record Book," such as all teachers there were required to keep and which contained the names and addresses of all children enrolled in a teacher's class, the names of the parents, the daily attendance of the children enrolled, and any transfers occurring. The first book which was signed by Miss Schmidt was in September, 1936. All of the books of Miss Schmidt from 1936 to 1941 were found at the Lafayette school.

[6] The writer wishes to express his sincere appreciation to Dr. Herold C. Hunt, General Superintendent of the Chicago Public Schools, and his staff, for permission to visit schools, interview teachers, and examine critically the case records from the files of the Bureau of Child Study.

3. The Lafayette school had one class for mentally retarded children prior to the fall of 1936. At that time the school officials organized a second class for mentally retarded children. In 1936–37 one class at the school was taught by Miss Schmidt. The other class was taught by another certified teacher, hereafter referred to as Miss X. The names of the children in these classes could be found in the respective "Teacher's Record Books."

4. In 1938 the Chicago schools reorganized services for mentally handicapped children and organized at the Lafayette school what was known as a "lower vocational center." Up to that time the classes for the mentally handicapped children in this school had been primary classes and consisted of both boys and girls. In 1938 only girls were included in these special classes. At that time, also, a third class was organized, thus giving three classes for girls of lower intelligence at the Lafayette Elementary School.

5. An attempt was made to discover what schools were designated as center A, B, or C. No one seemed to know that Miss Schmidt at that time was "head teacher" or was planning and supervising all of the classes. Actually no "head teacher" is appointed in the Chicago schools for these centers. The principal is responsible for planning, organizing, and supervising these classes.

6. Although Dr. Schmidt had refused to furnish the present writer with the names of the 254 children it was found that at least the names of the children enrolled in her classes could be obtained. The names of other children enrolled in other teachers' classes at the Lafayette School could also be obtained from the "Teacher's Record Book."

7. The present study, therefore, assumes that the children who were under Dr. Schmidt's care were included in the study which she reports. Since the names of these children were acquired from the "Teacher's Record Books," it was relatively easy to check on their initial IQ's from the files of the Bureau of Child Study.

The first investigation dealt with Dr. Schmidt's class of 1936–37. The "Teacher's Record Book" for that year was taken to the Child Study Bureau and the files were searched for the records of these children.[7] Many of these records were in storage, and some of them were already on microfilms. Some of the children had had several Binet examinations before being referred to Miss Schmidt's class. *The last test before referral is the one used in this evaluation.*

Since Dr. Schmidt probably included the other teacher's children in her study also, a search was made for the files of all of the children in Miss X's class during Miss Schmidt's first year of teaching, 1936–37.

The records of a large majority of the children whose names were in Miss Schmidt's "Teacher's Record Book" and in the other "Teacher's

[7] The writer secured the services of Mrs. Helen I. Gorski and Mrs. Alice M. Kenner, of the Illinois Commission for Handicapped Children. These individuals were not in any way connected with the Chicago Public Schools. The assistants searched the case records in the Bureau of Child Study. The present writer personally read every record found and tabulated the IQ records of the children reported in this study.

Record Book" for the year 1936–37 were found and are listed in Table IV.

Table IV gives the IQ's of the children before they were admitted into Miss Schmidt's class. Column 1 consists of the IQ's of Miss Schmidt's children; column 2 of the other teacher's children.

TABLE IV

INITIAL IQ RECORDS OF CHILDREN IN SPECIAL CLASSES AT LAFAYETTE SCHOOL IN 1936–37

IQ's of Miss Schmidt's Class		*IQ's of Miss X's Class*	
60	76	69	41
74	64	64	77
76	71	45	55
81	79	64	
61	72	85	
46	74	58	
80	51	57	
92*	77	75	
82		71	
72		67	
27**		65	
Mean IQ 69		Mean IQ 64	
Range 27–92		Range 41–85	

* This child was diagnosed as a reading disability. The psychologist recommended "remedial reading instruction."

** Excused permanently from school after 42 days of attendance.

It will be noted that the initial IQ's correspond closely to the statistics of the Chicago schools given in Table III, and do not correspond to the range, distribution, and means as published by Dr. Schmidt for all of her children. The following conclusions can be drawn:

1. The children in Miss Schmidt's class in 1936–37 ranged in IQ's from 27 to 92. This does not correspond to the range of IQ's given by Dr. Schmidt as 27 to 69.

2. The one child with an IQ of 27 was in Miss Schmidt's class for forty-two days. At the end of that time he was permanently excused from school and was not again listed in Miss Schmidt's "Teacher's Record Book."

3. The mean IQ for Dr. Schmidt's group as listed in Table 4 was 69, which does not correspond with the initial mean IQ of 52.1 listed by Dr. Schmidt for her total group or to the mean of any of her subgroups (See Table I).

4. Over 50% of the children in Miss Schmidt's class had IQ's above 69. Dr. Schmidt does not record one child as having an IQ above 69 of her 254 cases

(See Table II). In fact, she records exactly 69 as the top IQ in each one of the five centers.

5. The distribution of IQ's for the other teacher's class appeared to be narrower than Dr. Schmidt's, ranging in IQ from 41 to 85. The mean IQ, however, is 64. When we combine these two classes as one group, we have an average IQ (67) which is similar to the average IQ shown in special classes throughout the city of Chicago as listed in Table III.

The children in this study were a mixed group of boys and girls as stated by Dr. Schmidt. The ages, however, do not correspond with the ages listed by Dr. Schmidt for the total group of 254 children.

As was stated earlier, in 1938 the reorganization of special classes transferred all boys out of the Lafayette school and three classes for retarded girls were organized at this school. At that time the principal was responsible for placing children in these "lower vocational centers" *and could place the children in the classes without psychological examinations.* In Miss Schmidt's "Teacher's Record Book" for the second semester, 1938–39, when the classes became stabilized, were listed 23 children. A search in the files of the Child Study Bureau revealed the fact that 16 of them had been previously examined by psychologists from the Bureau of Child Study. Some of these tests had been administered several years before referral to the special class. The IQ's of these children are given in Table V.

TABLE V

INITIAL IQ'S FROM RECORDS OF CHILDREN IN MISS SCHMIDT'S LOWER VOCATIONAL CENTER CLASS IN 1938–39

IQ	*IQ*	*IQ*	*IQ*
58	77	63	69
65	72	64	65
80	56	67	70
79	80	75	66
	Mean IQ 69		
	Range 56–80		

It will be seen from Table V that the IQ scores of the children who were in Miss Schmidt's class in 1938–39 do not correspond to the data which she presents for the group as a whole (Table II). The following discrepancies are found:

1. The range of IQ's for the 1938–39 class was 56 to 80. Six of these children had IQ's above 69. According to Dr. Schmidt's data no child had an IQ above 69 (See Table II).

2. The mean IQ for these 16 children as listed in Table V is 69. Again this mean IQ does not correspond to the initial mean IQ listed by Dr. Schmidt in Table II.

3. Actually, the initial IQ's of the children in the classes investigated appear to agree more closely with the final IQ's listed by Dr. Schmidt after three years of training (71.6) and as shown in Table I, column 2.

The investigation did not continue beyond the recording of the data from the sample classes listed in the afore-mentioned tables. It would be relatively easy for any investigator to check all of the children taught by Miss Schmidt and recorded in her "Teacher's Record Book." Since the data presented in Tables IV and V for the sample classes are at such variance with Dr. Schmidt's data, it was not thought necessary that further investigation should be made.

Additional Discrepancies

In interviewing numerous people in Chicago, looking up data, and comparing with statements made in Dr. Schmidt's monograph, the following discrepancies were found:

1. Miss Schmidt writes as follows on p. 18 of her monograph:

The instructional staff included 11 teachers in addition to the writer . . . . The writer served each of the 3 centers as "head teacher," in which her duties were the planning and supervision of the educational program for the special center, and also as teacher of the social studies and the graphic and language arts.

To determine how a teacher can be employed as a full time teacher at the Lafayette school from 1936 to 1941, as determined from the personnel record, and supervise three centers and 11 teachers, the writer interviewed a number of individuals who were at the Lafayette school at the time of Miss Schmidt's study. The supervisor of special classes, three former teachers, the acting principal[8] (who had previously been an elementary teacher at the Lafayette school since 1934), and one psychologist who had tested at the Lafayette school during the years of the experiment were interviewed individually. None of the persons interviewed seemed to know anything about a "head teacher." According to these individuals "head teachers" are not appointed in these centers and never have been appointed in these centers. Furthermore, the teachers stated that they were responsible only to the principal of the school who assisted them in organizing and planning the curriculum for these special classes. They were very surprised to hear that Miss Schmidt had supervised them or had planned their work for them. If Miss Schmidt was "head teacher," planner, and organizer, no one of the individuals interviewed at the Lafayette school had any realization of that fact.

[8] The principal of the school was deceased in 1947.

2. Miss Schmidt describes Group I (p. 9) as:

> all those enrolled in Center A from November, 1935, to June, 1938 . . . .

Apparently Miss Schmidt claims to have begun the experiment in November, 1935. The personnel record, and the "Teacher's Record Book," show that Miss Schmidt was appointed as a probationary teacher in September, 1936. It is possible that she may have taken records of the children tested a year before.

3. Dr. Schmidt refers to the children in her study as "feebleminded." A perusal of the records in the files of the Bureau of Child Study showed that the psychologists did not classify any of the children as "feebleminded," as claimed by Dr. Schmidt. Children who range in IQ's from 50 to 80 are usually not classified as feebleminded. The policy in the Chicago Public Schools is not to classify children into categories such as "feebleminded," "moron," etc. They are listed as "children with retarded mental development," or with "slow learning ability," "who can profit from instruction in the special divisions." *The classification "feebleminded" used frequently by Dr. Schmidt in her monograph is Dr. Schmidt's own designation.*

The term "feebleminded" is a legal term used by courts in the commitment of mentally deficient children to institutions. It generally means that the child is uneducable, incurable, and that he requires supervision and support for his existence. Children referred to special classes are usually of the educable type who can become self-supporting. The classification of the children in Dr. Schmidt's study as "feebleminded" is not only contrary to the expressed policy of the Chicago Public School's Bureau of Child Study, but also contrary to current professional theory and practice.

4. Dr. Schmidt asserts (p. 21):

> . . . the average reading achievement for the total group was just below that of the child beginning the last half of first grade.

In spite of this lack of reading ability Dr. Schmidt administered and recorded scores on tests such as the Bernreuter (see Table I), whereas it takes a child of high school reading ability to read and understand the questions on the Bernreuter test. It is difficult to understand how such data are included in the study.

5. The data in Table I show some inconsistent statistical tabulations. Note, for example, column 2 under "Stanford Binet." Groups I, II, and III show mean IQ's of 71.6, 72.0, and 76.2 respectively. Yet the mean for the "Total" is given as 71.6. Note also under "Vineland," column 3, scores of 107.2, 84.8, and 83.8, averaging 107.2. The same question can be raised for column 3 under "Bernreuter" where −123.0, −12.6, and −12.5 averaged −123.0. The writer finds it difficult to follow such statistics.

6. On page 6 of the monograph in discussing the referral policy of the Chicago schools, Dr. Schmidt asserts:

> Those children so referred who were found to have an intelligence quotient below 70, on the basis of the individual examination, were assigned to a special class or special center.

For this statement she gives as her authority the Annual Report of the Superintendent of the Chicago Schools.[9] Actually this report (p. 289) states:

> Children whose intelligence quotients, as a result of individual psychological examinations, fall between fifty and seventy-five are assigned to ungraded classes.

This quotation, concerning the IQ range necessary for referral to special classes, together with the statistics for special classes in Chicago (Table III) do not correspond with Dr. Schmidt's statement of the policy of the Chicago schools, or with the range of IQ's as she reports them (Table II).

SUMMARY

Dr. Schmidt has presented data from a group of 254 children supposedly classified as "feebleminded" showing an increase in mean IQ scores from 52.1 to 71.6 in a three-year training period, and a further increase in IQ to 89.3 after a five-year post-school experience. The present survey confined itself to (1) a comparison of Dr. Schmidt's initial IQ data with the statistics of the Chicago Schools for such children, (2) a check of the initial IQ data from several samples of children enrolled in Miss Schmidt's classes in Chicago, and (3) other additional discrepancies. It was found that:

1. The data presented by Dr. Schmidt did not correspond with the data from the statistics for special classes in the City of Chicago.

   a. The mean IQ for children recommended for special classes in Chicago for the years 1937–40 is consistently 68 or 69; whereas Dr. Schmidt reported an initial mean IQ of 52.1 for her total group.

   b. Approximately 50% of children recommended for special classes in Chicago have IQ's above 69. Dr. Schmidt does not record one case out of the 254 in her study as having an IQ above 69.

2. Although Dr. Schmidt had refused the writer the names of the children in her study, he was able to secure the names of some of the children from "The Teacher's Record Book" found in the school in which she had taught. The IQ's of the children from several classes taught by Dr. Schmidt revealed a lack of correspondence between the

[9] JOHNSON, W. H. *Annual report, Superintendent of Schools.* 1939–40. Chicago: Board of Education, 1941.

data found in the files of the Bureau of Child Study and the data reported by Dr. Schmidt.

a. The initial mean IQ as recorded in the case records in the Bureau of Child Study for the children in the sample classes studied was 69, 64, and 69 respectively. Dr. Schmidt reported a mean IQ of 52.1 for her group as a whole.

b. The distributions of IQ's for both data again showed marked variations. Approximately 50% of the children studied by the writer had IQ's above 69; whereas Dr. Schmidt does not report any cases above 69.

c. The means and distributions of IQ's in the classes examined appeared to correspond more closely to the means and distributions of IQ's for the Chicago special classes (Table III) than to Dr. Schmidt's data (Table II).

3. No evidence was found that Dr. Schmidt was "head teacher" and supervisor of teachers in three centers in Chicago. Other discrepancies have been pointed out in the text, such as the beginning date of the experiment, the use of the Bernreuter and other tests with children who were reading at the first grade level, the erroneous statement concerning the IQ range used for referral to special classes in Chicago, etc.

In view of results presented and until the questions raised in this paper are satisfactorily answered it is regrettable that newspaper releases, and articles in popular magazines have appeared from time to time, such as: Blake Clark, "They are Feebleminded No Longer," *Reader's Digest*, September, 1947, pp. 111–115; Edith M. Stern, "Feebleminded Children Can Be Cured," *Woman's Home Companion*, September, 1947, pp. 34–35 & 156–158. These articles in general state that at long last a scientist has produced an educational procedure that will change children from the status of "feeblemindedness" to the status of "normalcy" in intellectual, social, and personality traits.

A REPLY

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The preceding evaluation[1] has done a service in clarification. Since 1939 it has been known to those concerned with the development and presentation of the study in feeblemindedness that it would not be validated through the Chicago Public Schools. The critical review now presented sets forth some of the points of conflict for analysis and interpretation.

Significant points of study in the evaluation can be summarized as: (1) nature of scientific method in research; (2) comparison of report with available published report; (3) bases for critical analyses; and (4) responsibilities of researchers.

Scientific Method in Research

The ultimate value of scientific research lies in its implementation through duplication of method by a variety of persons in a variety of locales. Method is useful or valid only when it can be applied by many people to secure similar or identical results. Specialized "methods" which cannot be transmitted to others through training to produce those results are the tools of miracle men, rather than trained scientists and therapists. This is the basis of the "Prefatory Statement" in the monograph.[2] In accord with this understanding of scientific method, the only person who *cannot* validate the scientific nature of the study through duplication is its writer.[3]

There are now under way four duplications of the reported program, which were indicated in the Prefatory Statement to be then at the point of initiation. They are, in order of date of beginning, Kirkland Lake, Ontario (Feb., 1947); The Laboratory School, Terre Haute, Indiana (April, 1947); The School of the Chimes, Baltimore, Maryland (Sept., 1947); and the Ann J. Kellogg School, Battle Creek, Michigan (Oct., 1947).[4] The projects at Terre Haute and in Battle Creek are in

[1] Kirk, Samuel A. An evaluation of the study by Bernardine G. Schmidt entitled: "Changes in personal, social and intellectual behavior of children originally classified as feeble minded." *Psychol. Bull.*, 1948, **45**, 321–333.

[2] Schmidt, Bernardine G. Changes in personal, social and intellectual behavior of children originally classified as feebleminded. *Psychol. Monogr.*, No. 281, 1946, **60**, No. 5, p. iii.

[3] See p. 322 of evaluation.

[4] These projects are under the following directors, respectively: Dr. C. E. Stothers, Supervisor of Auxiliary Classes, Toronto, Ontario, Canada; Dr. Byron Westfall, Principal of the Laboratory School, Indiana State Teachers College, Terre Haute, Indiana; Dr. Harry F. Latshaw, Director of Special Education, Baltimore, Maryland; Mrs. Doris J. Klaussen, Principal, Ann J. Kellogg School, Battle Creek, Michigan.

the public schools of their cities; the Kirkland Lake and Baltimore schools are under private auspices.

The concern voiced in the evaluation, that methods in duplication may not be accurate, has been voided by one of several means: (1) the study of those methods with the present writer by the directors of the projects; (2) the training of the teachers by the writer; (3) periodic consultant service by the writer during progress. Reports of progress in each of these duplications are not yet complete enough for publication, but regular correspondence with their directors and teachers indicates satisfaction on their part with results to date. The reader is referred to them for further definite data.

Comparisons with Other Available Published Reports

The statement has been made, but not documented, in the evaluation, that: "This study has been repeated many times without the results shown by Dr. Schmidt's data" (p. 323). In the absence of reference to check the authenticity of this statement, an examination was made of published research, and report of no such repetitions and their failure could be found. There has been minimal reported experimentation in educational method in school programs for the mentally handicapped; however, absence of report of research can hardly be accepted as evidence that experimentation has been conducted with negative results, since scientific investigation carries the responsibility for report of results without bias.

One of the preliminary tests of the probable validity of new research is its comparison with that already available. In general, it can be expected to provide similar results *if procedure has been similar;* it should be expected to show differing results, if procedure has been different. No claim has been made that any *part* of teaching method used in this experimental study was completely new or original, but only that it *put into practice an integration* of methods usually verbalized as desirable in school programs, but not always implemented.[5]

It has been pointed out earlier the paucity of published experimentation in this field, but none that has been published reports negative results. On the other hand, the available literature supports the current study. In 1923, Anderson and Fearing studied a group of 298 who had been out of school up to six years, and found about eighty percent to have made satisfactory adult adjustment.[6] In 1932 the Children's

[5] Schmidt, B. G., *op. cit.*, pp. 45–46; and direct quotation, *Baltimore Sun*, Sept. 21, 1947; " . . . (the methods) are the same as of all good general education."

[6] Anderson, V. V., and Fearing, M. A., *A study of the careers of three hundred twenty-two feebleminded persons.* New York: National Committee for Mental Hygiene, 1923. Pp. 213.

Bureau of the United States Department of Labor reported on approximately 1,000 pupils who had left special classes and found generally good adjustment and vocational competency for slightly more than 86% of the cases.[7] Beaman, in 1932, reported a follow-up study of mentally handicapped boys, and found consistently satisfactory post-school adjustment.[8] The classic report of Locust Point, in Baltimore, made by Fairbank reporting on sub-normal children seventeen years after leaving their home community, showed 75 percent of the group to be fully self-supporting and independent.[9]

Interest in intelligence test retests did not become strong with researchers until the beginning of the Iowa reports, although Fairbank included some retests in her study. However, Kephart, reporting in 1940, indicated that over a one-and-a-half year period of specialized opportunities, fifty percent of his small group of retarded 15 to 18 year olds had increased more than 10 points in IQ, and 25 percent had gained 14 points or more.[10]

In 1944, Muench, a student of Rogers, reported a retesting program of a small group of mental defectives after eighteen years, taking as his subjects cases diagnosed in 1925 by Adams, working under the direction of Doll. He searched the city to determine their current test level, and economic and social adjustment. While only a small number could by that time be located, the mean change in IQ was 27.2 points, and all showed socially acceptable marital and vocational adjustments. Their then current salaries were strikingly similar to those reported for the Chicago group taken within the same year.[11]

In addition to these reports of mentally defective individuals, the last decade has seen much research on the general aspects of the nature-nurture controversy, which need not be reviewed here.[12] It should be mentioned that, since the nature of intelligence is basic to the problem of the development of the mentally handicapped, findings in this general body of research relative to improvement in mental ability due to changing school environments and educational programs can be legitimately considered as confirming research in relation to the current study.

[7] CHANNING, A., *Employment of mentally deficient boys and girls.* U. S. Children's Bureau, Pub. No. 210, Washington: Superintentdent of Documents, 1932. Pp. 37.

[8] BEAMAN, F. N., *An experimental curriculum for special classes.* Master's thesis, Northwestern University, 1932. Pp. 312.

[9] FAIRBANK, R. E., The subnormal child seventeen years after. *Mental Hygiene,* 1933, 17, 177–208.

[10] National Society for the Study of Education. Yearbook XXXIX. *Intelligence: its nature and nurture.* Bloomington, Ill.: Public School Publishing Co., 1940, p. 228.

[11] MUENCH, GEORGE A., A followup of mental defectives after eighteen years. *J. abnorm. soc. Psychol.,* 1944, 37, 407–418.

[12] National Society for the Study of Education, *op. cit.,* 1940.

In the light of published research which substantiates the current report, and in the absence of contradictory negative report, it seems in order to refute the undocumented statement in the evaluation that: " . . . these results are in sharp contrast to current professional opinion" (p. 322), and, that "A recent report . . . has presented data far beyond that which has ever been presented by any other writer" (p. 321).

Bases for Critical Analyses

An evaluation of report can be expected to examine critically all known phases of a study in order that biases for and against, and information unavailable in one source but open in another, may be assembled and interpreted with relation to each other in order to arrive at valid conclusions of critique. Particularly is such exhaustive and careful procedure desirable when the subject of analysis is controversial.

Records and recorded data are admissible in critical analysis if they are accepted as, or can be established as, reliable, accurate, and complete. Oral report is of questionable validity in the establishment of events of some time past, unless that report was transcribed as taken at or near the time under question. With all such sources of information, internal as well as external consistency in report is desirable in the sifting of truth.

The evaluation of the current study relies solely on information secured from the Chicago Schools. In view of the history of controversy of this study with that source, it is regrettable that the investigator did not make use also of the following sources of additional information, suggested to him in correspondence by the writer at the time of the beginning of his evaluative investigation:[13] (1) members of the examining committee, and the Dean of the School, of the University which directed the study;[14] (2) individuals of leadership in the field of Special Education who have within the past three years actively investigated the study, through the Chicago Schools, *and also* through other sources.[15]

[13] Correspondence referred to in the evaluation, p. 326.

[14] Specific referral was made to Dr. Robert H. Seashore, Head of the Department of Psychology; and Dr. J. Monroe Hughes, Dean of the School of Education, Northwestern University.

[15] Particular referral was made to Dr. Harry F. Latshaw, Director of Special Education of Baltimore, Md., who in May, 1946, and again in December of that year made intensive study of the case files of the children in the experimental program; then went to the Chicago Schools, searched records in the schools and interviewed various personnel; and, finally, conferred with officials and checked records at Northwestern University to obtain further data to help establish the authenticity of the study. A report of the long-term evaluation of the experiment was reported in the *Baltimore Sun*, Sept. 21, 1947, concluding with the statement: "The two Baltimoreans found sufficient proof of success to warrant introduction of the method here." It was after this study that the woman

These sources were available to the investigator, and were suggested to him, but either they were not consulted by him, or their evidence was selectively deleted, since no reference to these sources appears in the evaluation. Since exhaustive and careful investigation was the stated objective of the evaluation, it is worthy of note that the investigation was begun sometime after January 26, 1948, and concluded, assimilated, interpreted, and put in publication form by the first week of April.[16] While much can be done by some individuals in a short space of time, a total calendar period of about eight weeks for the full investigation, synthesis, and evaluative report of a study twelve years in such process,

TABLE I

ENROLLMENT IN SPECIAL CLASSES FOR 1935–40, AS TAKEN FROM ANNUAL REPORTS OF SUPERINTENDENT OF SCHOOLS

	1935	*1936*	*1937*	*1938*	*1939*	*1940*
Opportunity Rooms	256	184	27			
Ungraded Rooms	4,432	4,685	4,428	3,877	3,716	3,380
Lower Vocational Centers			8	1,039	1,398	1,556
Total reported from Superintendent's Report	4,688	4,869	4,463	4,916	5,114	4,936
Total reported from Table III in evaluation			1,994	2,132	1,786	1,595
Unexplained difference			2,469	2,784	3,328	3,341

seems a little brief, especially when all sources of data have not been approached.

Much of the comparisons of records in the evaluation centers around figures reported therein for children recommended for ungraded divisions. This table, according to its documentation, is reproduced from an official publication of the Chicago Board of Education.[17] Assuming

who later became director of the School of the Chimes came to study under the writer in teacher training courses in this field, and returned to Baltimore to open the new school.

Similar study of records in the study, and in Chicago and at Northwestern University, was made by Dr. C. E. Stothers, Inspector of Auxiliary Classes, Department of Education for the Province of Ontario, Toronto, Canada, and his staff in February, 1947.

[16] Correspondence to Dr. Kirk from the writer was dated January 26, 1948; copy of the finished evaluative article was submitted to the writer from the editor of THIS JOURNAL under date of April 7.

[17] Page 325 of evaluation. Cf. *Bureau of Child Study and the Chicago Adjustment Service Plan.* 1940–41. Board of Education, City of Chicago.

it has been reproduced accurately, it should be expected to be in agreement with other official publications, from the same source, concerning the same figures, for the same years.

However, examination of the figures given for the total number of children recommended for ungraded divisions for the years 1937, 1938, 1939, and 1940, when taken from their respective Annual Reports of the Superintendent, do not coincide.[18]

According to the figures obtained from the statistical tables in the Annual Reports of the Superintendent, it appears that the figures in Table III of the evaluation account for only a small percentage of the children enrolled in special classes in Chicago during those years: in 1937, 44%; 1938, 43%; 1939, 34%; and 1940, 32%.

The fact that the table is based, in each year, on less than half all children so enrolled makes it impossible to use its distribution of intelligence quotients as a valid basis of comparison. Since no explanation is available concerning the lack of correspondence in published enrollment figures from the same source, it is impossible to estimate the probable distribution of the missing enrollment population, nor to assume a selective basis for their omission from the table numbered "Table III" in the evaluation. Furthermore, in the context of the Annual Reports (separate from the cumulative tables presented in Table I) are found other conflicting statements concerning enrollment figures:

For the year 1935, "There are now 234 Ungraded Divisions of classes, caring for 5,875 children."[19]

But, from Table I, in 1935, according to the same volume of official reports, there were only 4,688 children so enrolled. No mention nor explanation is made of the discrepancy of 1,187 children, considered enrolled on page 252 of the report but omitted from the cumulative statistical report on pages 347 and 348 of the same volume.

Similarly, for the year 1940, the context of the Report states: "There are now (1940), 186 ungraded classes, where 3,720 mentally retarded children are afforded a greater possibility of success and adjustment."[20]

But, according to the figures for Table I, taken from pp. 540 and 541 of the same volume, there were 4,936 children so enrolled, an unexplained discrepancy of 1,216 children; and, according to Table III of the

[18] Excerpted from JOHNSON, WILLIAM H., *Annual report of the Superintendent of Schools*. 1940–41. Chicago: Board of Education, pp. 535–536 (Tables 28—Sections E, F, and G).

[19] JOHNSON, WILLIAM H., *Annual report of Superintendent of Schools*. Chicago: Board of Education, 1936, p. 256.

[20] JOHNSON, WILLIAM H., *Annual report of Superintendent of Schools*. Chicago: Board of Education, 1941, p. 477.

evaluation, there were only 1,595 so enrolled, a discrepancy of 2,125.

Again, despite the constantly repeated statement in the Reports that children who are recommended into the ungraded divisions, or special classes, have intelligence quotients between 50 and 75,[21] Table III of the evaluation lists varying numbers of boys and girls recommended who are both *below* and *above* these limits.

These continued discrepancies show a lack of internal consistency within the published statistics of the Annual Reports of the Superintendent, emanating from the Chicago Board of Education (up to and including 1941), and external inconsistency in relation to other volumes of statistical data concerning the schools, and published from the same source. With such variance evident, it is difficult to accept such records as validating media for substantiation of critical research.

In the evaluation report, a number of children whose names appeared on Record Books of the special classes were checked as to initial IQ's on record in the Child Study Bureau (pp. 326–330). Again, despite the statements in the Superintendents official Reports that only children from 50 to 75 IQ are so placed, of 49 children so checked (Tables IV and V of evaluation) 14, or over one third, were found to have IQ's *higher* or *lower* than these limits. The tests checked by the evaluator, in some cases, were "administered several years before referral to the special class" (p. 329). And yet, persistently in the Annual Reports is found the statement that children are re-examined every two years.[22]

If the published documents are to be believed as accurate statement of fact, the question must be asked: Why were the examination records seen by the investigator those administered "several years before referral"? Also, why were some of the records, even of those selected by the investigator, not found for checking,[23] since the statements are constantly repeated that no child can be placed in an ungraded division except after examination and recommendation by the Child Study Bureau?[24] Although the investigator says, on p. 329, that: "At that time the principal was responsible for placing children in these lower vocational centers, and could place the children in the classes without

[21] JOHNSON, WILLIAM H., *op. cit.*, 1939–40, p. 289; 1940–41, p. 478.

[22] JOHNSON, WM. H., *op. cit.*, 1938–39, p. 262; *op. cit.*, 1939–40, p. 232.

[23] Pp. 327–328 of evaluation: "The records of a *large majority* . . . . were found and listed"; and p. 329—"A search in the files of the Child Study Bureau revealed the fact that 16 of them (total 23) had been previously examined by psychologists from the Bureau of Child Study." (Italics and parentheses mine.)

[24] JOHNSON, WM. H., *op. cit.*, 1936, pp. 253; 1939, p. 228, p. 262; 1940, p. 400, p. 477; 1941, p. 478.

psychological examinations," his statement is undocumented, and published records for that year (1938) state otherwise:[25]

> Studies have been made in the lower vocational centers at the time of their organization, to guide the assignment of individuals and investigate factors that block learning and interfere with wholesome personality development. (Reported in section describing activities of Child Study Bureau.)

From the foregoing analysis of internal and external inconsistency of records, and published documents based on such records, it is understandable why they can be expected to offer little assistance in scientific evaluation. Another source of record considered basic by the evaluator is the Teacher's Record Book, but such is not always the case because "paper membership" still obtained in those years. "Paper membership" is a device whereby a child remains in one grade or room in actuality, but his membership is transferred, on paper, to the records of another grade or room. In this way, his permanent records may be maintained under the name of a teacher, a grade, or a room with which he has not had actual experience. The children reported in the monograph were those actually taught in the centers, and not necessarily those whose memberships were so listed.

With the exception of the data obtained from contradictory sources of record, much of the information of the evaluator is undocumented.[26] Since the principal who was in charge of the building was deceased in December, 1947,[27] and since the former Assistant Superintendent of Schools in charge of Special Schools and classes has since retired, it is difficult to decide the sources of this information. At best, it must be estimated that it came from oral statements of persons who may have had varying degrees of contact with the special class organization and function at the time of the experimental program. Undocumented oral statements, drawn from a thirteen-year memory span, from persons not previously directly responsible for knowledge of the information upon which they are questioned, can be expected to have a share of inaccuracy and incompleteness.

The personnel records seem to have been misinterpreted. According to the evaluative report, the writer did cadet teaching during the second semester of 1935–36; cadet teaching was actually done during the second semester of the school year 1933–34. Assignment on permanent tenure

[25] JOHNSON, WM. H., *op. cit.*, 1938, p. 262.

[26] P. 327, beginning with item 3, and continuing through item 5, of evaluation.

[27] See footnote, p. 330, of evaluation.

was in September, 1936; but that was not "first assignment." It is difficult to interpret accurately the personnel record as presented in the evaluation, since the term "probationer" was not used (at that time) with teachers assigned to the Chicago Schools, at least with the knowledge of the candidates.

It is difficult to consider the content of the evaluation in the light of the previously presented bases for critical analyses. It has failed to investigate all but one source of information; it has used only records and published data which are themselves inconsistent, both internally and externally; it has accepted as a basis for critique very limited, partial data, with no explanation of its part basis, nor no consideration of the bias such selective data lends to its conclusions; and it has included oral report, undocumented, based only on incidental memory over a span of more than a decade.

Responsibilities of Researchers

The person who undertakes research assumes at the same time a number of responsibilities. To his profession and co-workers, he owes honest presentation of accurate data, careful interpretations, and pertinent conclusions. In addition, when such research is based on human lives, he owes, likewise, major responsibility to those whose co-operation has made it possible to conduct the study while in progress. In such research as the latter, it can be accepted that experiment in human development is conducted with one major aim: the improvement of that development. When responsibilities incurred to these groups by research conflict, the researcher must himself develop a hierarchy, and determine where duty lies.

In this experiment in feeblemindedness, the first objective was provision of opportunity for improved living for the boys and girls who were the subjects of study. To the extent that the research can be made available for further implementation, without jeopardizing the improvement won for these individuals, it should be circulated. If and when interest in this completed research becomes so insistent that it interferes with the privacy of these individuals, now young men and women, then their welfare must be preserved at the expense of the interest of any other group. For this not to be so would be to relegate the welfare of the boys and girls so studied, to an importance other than paramount.

The research study has been available under authorized acceptance or publication, since June, 1945. Since that time records and files have been available, and have been perused by many interested professional groups and individuals. However, when the evaluator of the currently

presented critical report wrote to ask for the names and addresses of the individuals, it was necessary to refuse that information, since continued spot-light scrutiny cannot help these young men and women preserve the adjustment they have worked so hard to win.

This explanation was given the evaluator when he requested that the identities be disclosed. (He has quoted only one sentence of the letter sent him by the writer.) If human lives were not the cost, they could be so disclosed. Under the circumstances, these records are case histories, and, as such their individual identities are confidential. This is the obligation of all who undertake case-study technique, and it is with this understanding that much of the most useful data are accumulated, data which could not be, and would not be, given were it not for the trust of confidence.

The study in feeblemindedness was first attempted to investigate improved educational opportunities for seriously mentally handicapped boys and girls. It will now serve its aim best by duplication so that others may experience the same opportunities.

DISCUSSION OF STONE'S NOTE ON THE LAW OF EFFECT[1]

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It is indeed "not an ideal technique to take specific quotations out of context" (2, p. 151). Stone has attempted to juxtapose selected quotations from my review of the law of effect (1) and from one of Thorndike's publications (3) in order to show that I did not do justice to Thorndike's position on the effects of punishment. Specifically he takes me to task for overstating Thorndike's position and claiming unjustly that Thorndike denies the effectiveness of punishment in general. His quotations are intended to prove that Thorndike does consider punishment an effective condition of learning, at least within certain limits. The apparent contradiction between Thorndike and my interpretation of Thorndike would have largely disappeared if Stone had chosen to quote the following passage from my paper which contains virtually all the points with which he has confronted me:

> The results of the experiments led Thorndike to conclude that punishment instead of weakening or "stamping-out" the wrong response, may have a variety of effects depending on the specific nature of the annoyer and the propensities of the organism. The important point is that what an animal is led directly to do by an annoyer *may or may not*[2] make the repetition of the punished response less likely. If punishment does lead to the elimination of a response, its action is *indirect:* it leads to variability of behavior, thus increasing the opportunities for the occurrence of the correct response which is then reinforced by the direct action of the satisfier (OK reaction) (1, p. 502).

Admittedly, a "battle of quotations" cannot help but become increasingly futile, but since a juxtaposition of quotations was used in the original rejoinder, it seemed only fair to set the record straight.

It seems, then, that the quotations cited by Stone do not make any additional points. As to the question whether Thorndike tended to over-generalize from his results, such a question can certainly not be settled by use of quotations. Review of a voluminous literature covering many years led me to conclude that he did, since he seemed to apply the conclusions of his experiments to punishment in general. My con-

[1] STONE, G. R. A note on Postman's review of the literature on the law of effect. *Psychol. Bull.*, 1948, **45**, 151–160.

[2] Italics added at this writing.

cern was with fitting Thorndike's view into the general picture of current learning theory and I must disagree with the suggestion that evidence obtained in non-Thorndikian situations is irrelevant (2, p. 153).

If it is permissible to end on a personal note, I fully share Stone's admiration for Thorndike's work. Nor do I feel that documented criticism should be construed as a lack of respect.

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CORRIGENDA

In my article entitled "The detection and treatment of accident-prone drivers," which appeared in THIS JOURNAL in November, 1946, **43**, 489–532, two serious errors have been discovered besides those which have already been reported. They are:

P. 512, par. 3, line 12: For 18 put 13.
P. 512, par. 3, line 13: For 19 put 10.

H. M. JOHNSON.

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SUGGESTIBILITY AND NARCOSIS—A REPLY TO EYSENCK

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In a recent review of the subject of psychotherapeutic counseling which appeared in this journal (3) I referred briefly to a paper by Eysenck and Rees (2) on suggestibility and narcosis. Later Eysenck (1) criticized the reference as being seriously misleading and falling short of "any reasonable standard of accuracy." All reviewers are faced with the problem of attempting to condense articles in such a manner as to preserve their significant meaning in the fewest possible words. This task may be very difficult in articles in which the authors themselves have either been unclear in their statements or confusing in their organization. This we consider to be true in the case mentioned above.

Eysenck criticizes our concluding from their original article that they considered neurotic individuals more suggestible than normals, and states that none of their subjects were normal persons, all being classified as neurotics. However *nowhere* in their article do they say that *all* their subjects were neurotics, and they make four references to contrasts in suggestibility between neurotics and normals. In only one of these cases are these references clearly identified as relating to *previous* studies. Furthermore, although Eysenck says in his criticism that in another paper on the subject adequate criteria for neurosis are given, that does not change the fact that the reader was given no indication of this in the article reviewed.

Eysenck corrects the statement that there were only 30 subjects, indicating that there were 50, and states that "these facts were made perfectly plain in the original paper." To the latter statement I take exception. The exact number of subjects was far from clear. In the section describing the experiment, only twenty subjects are mentioned, and it is implied that the same subjects were administered both sodium amytal and later, nitrous oxide. More confusing is the fact that kymographic records were reproduced in detail for 30 of the subjects, but for none of the others. It would have been desirable to have used the space occupied by some of these kymographic records for the presentation of a table which would clearly show what subjects received the various treatments, and what responses characterized each group.

The authors make two references to the highly significant character

of their data, and although these may be entirely justified, there is no indication of the procedure by which these results were computed. That a mathematical check was applied, we do not dispute, but we feel that it would be desirable for authors of scientific articles to substantiate their claims to validity with more thorough descriptions of their methods of statistical analysis. Further, these authors did not indicate on what basis they chose their subjects in order to obtain a representative sampling.

It may have been a weak presentation of the authors' conclusions when we stated that suggestibility is not greatly affected by the use of narcotics, whereas the authors said that this is true of non-suggestible persons, but not of suggestible ones. This statement resulted from difficulty in interpretating the authors' frequent references to the high correlation found to exist between traits of neuroticism and primary suggestibility. There seems to us to be an inconsistency between saying that there is a high correlation between primary suggestibility and neuroticism and reporting results obtained on 20 non-suggestible individuals who are also considered to be neurotic, although they were not clearly identified as such. The fact that no normals were used in this study, although this was not clearly indicated in the paper, justifies considerable question as to whether the phenomenon of suggestibility in neurotics may not be a different process from that of suggestibility in normals, but this limitation of the results of this study was not made.

Further confusion existed in the authors' indication that some of the increase in suggestibility could be due to practice effect and/or the suggestive effects of the procedure of administration. The investigation of this alternative hypothesis was based on a control group of only 10 subjects. No matter what statistical significance an author quotes, readers may wonder about accidental selection in groups of this size. And this is especially true when the small group at least in part supports an alternative hypothesis.

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2. EYSENCK, H. J., & REES, W. L. States of heightened suggestibility: narcosis. *J. ment. Sci.*, 1945, **91**, 301–310.
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BOOK REVIEWS

MURPHY, GARDNER. *Personality: A biosocial approach to origins and structure.* New York and London: Harper, 1947. Pp. xii + 999. $5.00.

In modern psychology, the term "personality" has acquired either of two meanings, individual differences, or "the thing which all personalities, as such, possess—the thing that marks off a personality from all other objects, such as a tree or a triangle." In Murphy's remarkable new book, the emphasis is definitely on the second of these conceptions i.e., upon the dynamic organization rather than the differential aspects of personality. Or, to use the author's own words, it is "upon the general rather than upon the particular: we are more interested in formulating a working conception of personality than in endeavoring to define in detail the infinite variability of personalities" (p. 1). It has thus "not been possible to do justice to the quantitative problems revealed by psychometrics, by factor analysis, by ratings, and by questionnaires, or to personality tests or therapeutic and educational problems" (Foreword). Although, therefore, it is concepts and problems of general psychology that stand out in the presently discussed volume, it soon becomes apparent that, among these, emphasis is on those features which are the most promising determinants of individual differences. Thus, the book helps to bridge the threateningly widening gap between the approach to basic psychological principles, on the one hand, and the fields of application, such as clinical, differential, or social psychology, on the other.

The book is divided into six parts of approximately equal size. The first part endeavors to consider personality as a biological system, and covers such topics as heredity and physical growth, individual constitution, the "elementary biology of motivation" as given by tissue needs, biological motive patterns, and organic traits and their measurement. Selection of the material is somewhat casual in this part of the book, and few concrete data are given.

The second and third parts take up the two basic psychological functions of learning and of perception. Discussion of the place of learning in the acquisition of the behavior system that constitutes personality starts with the introduction of the concept of "canalization." The term as adopted from Janet acquires broad significance in Murphy's entire system and is widely applied throughout the book. It refers to the "process by which general motives (which are at first rather nonspecifically related to a class of stimuli) tend, upon repeated experience, to become more easily satisfied through the action of the specific satisfier than of others of the same general class" (p. 162). Experimental evidence on "familiarization" is cited as a case to the point, leading to the

discussion of "acquired tastes, wants, values" and their relationship to conditioning. The latter is subject to extinction whereas canalization "so far as we know" can "be broken . . . only by more powerful responses which in their essence block the energies originally established." However, "canalizations are in general free of interference from one another" (p. 167) and contribute, as is discussed later in the book (Chapter 30), to the establishment of personal "continuity." Moving on into psychoanalytic theory, canalization is linked to McDougall's "sentiment" and Freud's "cathexis" and "fixation." In the subsequent discussion of conditioning and the learning of symbols and values emphasis is on "the relative strengths of two incompatible tendencies" and on other aspects of conflict as well as on the hierarchical organization of trends and attitudes.

As befits the main theme of personality, the entire treatment of learning is thus slanted toward the motivational; the same holds for Murphy's approach to perception and thought as presented in the third part. Characteristically, it bears the title "The Personal Outlook." This penetrating essay on the "cognitive-affective system" admirably incorporates much of the available experimental evidence of the shaping of perceptual organization by our needs and personal prejudices (especially in a section on "autism"), and there is frequent reference to Werner's and Piaget's views regarding the fusion of the cognitive with the affective in early stages of development. Over and above the purely scientific aspects, Murphy's eloquent rallying to the support of the "dreamer," and of creative ingenuity in general, circumscribe one of the several issues on which the author may help to mold the cultural panorama of our time.

Parts IV and V deal with "The Self" and "Wholeness." The section on "psychoanalytic mechanisms" contains what the author considers "simplifications of Freudian doctrine in a form compatible with the systematic outlook already developed." There is "no place for a debate between psychoanalytic and other psychologies; rather, the aim is to see where the psychoanalytic contributes and enriches the system already sketched in simple terms." And, in preparing for the section on "compensations for inferiority," Adler's contributions are appraised as "valuable studies of the nature and functions of the ego, which, if examined empirically, involve little with which clinical or experimental observation can quarrel." As to the unconscious, "the ultimate dynamics appear to be closely related to those which appear at the behavior and the conscious levels; the main working principles seem to be supported rather than rejected" (p. 19 f.). And, in line with a general trend pervading the book, the author, in discussing the therapy of inferiority feelings, agrees to the opinion that "the problem is essentially situational": there must be a "discovery of the remaining security areas, . . . the inculcation of social feeling, . . . (and) the unmasking of poses" (p. 582).

Although the author stresses the importance of situational and cultural factors throughout the book, this emphasis is certainly not exclusive. His is not a situationism in the sense of assuming behavior forms specifically elicited by certain external situations, witness the comparatively broad space devoted to the discussion of the Self and of the "membership character" which behavior acquires in the context of personality as a whole. "A broad base for personal continuity has apparently been found, a base consisting of biological individuality, early canalizations and conditionings, ingrained perceptual habits, and a well-organized and rather stable structural whole. But personality does change, in matters both large and small" (p. 727). Murphy furthermore seems to ascribe more weight, in the formation of character, to the situations in early childhood than to the situations the individual has to meet in later life (see Chapter 37). This is one of the many common elements with psychoanalytic theory although Murphy differs from the orthodox psychoanalytic view as to the relative importance he ascribes to the present as contrasted to the past.

The chapter on wholeness contains, among other things, brief but pertinent and technical comment on such projective techniques as the Thematic Apperception Test and the Rorschach, as well as an informal but sympathetic discussion of free drawing, of graphology, and of play techniques in children. The chapter concludes with a section on "discontinuity and typology," containing discussion of "biotypes" along with some of the Freudian character types and of "socially defined" types. However, the source of discontinuity is seen to lie primarily in the changing image one has of oneself in different social contexts. In spite of all the recognition given to such internal factors as the inherent "style of life" or such relative constants as "frustration tolerance," there is somewhat more emphasis on the biological and the social than on the more properly psychological dynamics.

The book concludes with a series of chapters on the relationships between "Individual and Group" (Part VI). Discussion of group membership and social roles is followed by consideration of "canalizations upon the family" and of the family as mediator of culture, and of W. I. Thomas' so-called situationism. "The situationist requires that a study of situations that act upon persons should be at least as full and as systematic as is a study of the internal structures which respond to these situations. For the situationist, personality is the generic human response, the response which any human being has to make to a situation that is fully defined in terms of the role requirements of anyone who must function in that situation" (p. 877). This approach is briefly compared with Moreno's sociometry and psychodrama as well as with other social-psychological "field theories." Murphy himself adopts a more balanced interactionist view of the personality-and-culture thema. "This is the most inclusive integration of data which we shall attempt.

It is not the last word, though it is this book's last word, on our problem of personality. Throughout its presentation of the 'social science' and 'field' points of view, suggestions about the present characteristics of personality in the American scene will be hazarded" (p. 21). An example of the many lucid treatments of specialized topics in this chapter is the analysis of the authoritarian character.

The profound erudition and the synoptic knowledge of the literature in a wide variety of fields as commanded by the author, together with a unique ability to disentangle the network of interactions that shape modern man, have contributed to the creation of an extremely worthwhile book. Not only is there ample documentation of each major point from the experimental literature, but there is also a colorful array of case studies or material from cultural anthropology. It is such courageous synthesis rather than the development and elaboration of a particular point of view that is characteristic of the book. The general mode of approach will remind the reader of two previous books the author has written or to which he has contributed, *Historical Introduction to Modern Psychology* and the *Experimental Social Psychology*. It is perhaps due to the greater scope, the fluid state, and the shifting conception of the problem before us that the present book on personality is less systematic in the sense of tightly-knit orderliness than are the previous two. Yet, it is no less complete and no less integrated if one is willing to take an over-all point of view; and it has in common with them the flexible, synoptic presentation that many will recognize as the characteristic Murphy touch. A truly encompassing conception of personality cannot be divorced from the whole of psychology as it exists at the present time, and the book is in this sense best designated by calling it simply a "Psychology," by Murphy, as of 1947. Or, in the author's own words: "Psychology of personality would then be that particular kind of general psychology that emphasizes totality and the organic systematic relations which obtain within it" (p. 3). The very feature of seeing the psychology of personality as inseparable from general psychology, and of relating it to the biological as well as to the social sciences with the greatest sensitivity and receptiveness to concrete avenues of research, belies Murphy's statement that "no one person's evaluation of present data and systematic theories is of great value"; if there is anyone to succeed in such evaluation, it is Gardner Murphy.

ELSE FRENKEL-BRUNSWIK.

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KÖHLER, WOLFGANG. *Gestalt psychology*. New York: Liveright, 1947. Pp. 369.

Certain unwarranted statements of the publisher on the jacket notwithstanding, this is but a very slight, mainly stylistic, revision of the book that appeared almost twenty years ago. A behaviorist asked to

announce it could therefore simply refer to C. C. Pratt's earlier review in THIS JOURNAL (1929, **26**, 693–706). However, since then the formulation of systematic behaviorism has undergone significant changes. To illustrate this development rather than to flog dead horses, I shall reexamine some of Köhler's familiar contentions.

The first chapter is called "A Discussion of Behaviorism." Here is the gist of its *argument:*

1. Behaviorists take for granted that we all observe the same physical universe (of chairs, laboratory equipment, overt organismic behavior, etc.). These observations, public, communicable, and objective, constitute proper material for science. Also, they are the only such material available; for subjective experience so-called (percepts, emotions, volitions, etc.) is not thus public or, at least, communicable. As far as the other fellow is concerned, we may even doubt that it exists.

2. In all this the behaviorists are wrong or, to put it mildly, very naive. All our experiences—and they are all we have to start from—are, in a philosophical sense, subjective. Nor is there anything particularly objective about physics, except that the physicist, by the nature of his subject, may restrict his starting material to a certain subclass of his givenness (percepts of laboratory equipment and of pointer-scale coincidences). But the physical universe is, philosophically speaking, a construction.

3. There is thus no reason to exclude subjective experience from our starting material, the less so since it is at least immediately given and not, like the world of physics, a construction. Only if we adopt this broader basis can we achieve actual scientific success in psychology.

Historically, arguments as naive and untenable as (1) were actually proffered by some early behaviorists and are still occasionally repeated by people who do not enjoy Köhler's dangerous privilege of having been reared in a metaphysically saturated culture. However, (1) is not the argument of contemporary behaviorism. From its viewpoint a systematic *criticism* of Köhler's argument will proceed along the following lines.

1′. Like Köhler, the behaviorist starts from experience (though, unlike him, only from his own) and does not worry whether it is, perhaps, in some philosophical sense subjective.

2′. He notices that it is possible to construct, by using only part of what is thus immediately given to him, not only the physical universe but also the other fellow's "experiences." On the one hand, his subjects' conscious contents (their minds) are, therefore, as "real" to him as the world of physics. On the other hand, their being defined constructs properly reflects the fundamental fact that they are not among his own givennesses and, therefore, cannot occur among the starting material of a systematic analysis.

3′. There are indeed no philosophical grounds for excluding any data from the starting material. Behaviorism excludes some for what may be called practical reasons, because by excluding them nothing is lost and something is gained. Nothing is lost because every experiment that has ever been made or will ever be made, by a Gestalter, a Wundtian, or anybody else, can be stated in be-

havioristic language. Something is gained because only if we insist on so stating it can we be sure to achieve objectivity and reliability, not in any recondite philosophical meaning of those terms, but in that ordinary down-to-earth sense in which, to Watson's exasperation, the work of Würzburg and of the later Wundtians was neither objective nor reliable.

In the next chapter, "Psychology as a Young Science," a case is made for the indispensability of certain data *qua* data. Undoubtedly we often perceive a man's voice to be shaky or judge him to be angry. Yet these are givennesses of the kind strict behaviorists wish to exclude from their starting material. Against them Köhler *argues* as follows.

1. It would be foolish for a young science not to use such data as long as it does not have behavioristically defined concepts that correspond to them and, being of a quantitative nature, measure them.

2. Even if we had such concepts, however refined, what good could they do us if we did not know what, let us say, anger was, if we were not acquainted with its "nature," or, to put it quite bluntly, if we did not know its qualitative feel.

3. Enamored of the quantitative and neglectful of the qualitative, psychologists in general and behaviorists in particular waste much effort in carefully elaborating many meaningless concepts or, at least, concepts whose meanings they ignore.

Historically, this argument is a potpourri of familiar philosophical themes. And, to return a compliment, it strikes the contemporary philosophical analyst as rather naive. By way of systematic *criticism* I would say this. (1) is beside the point. To make shift with what one has is one thing; to make a methodological virtue out of our ignorance is another. The point of (3), abstractly considered, is well taken. The meaning, nature, or essence of a concept is indeed not its definition, but the sum total of the empirical laws (functional relationships) into which it enters with other concepts. But, and here we put our finger on the crucial equivocation, meaning thus understood is not the qualitative essence spoken of in (2). It follows that the behaviorist is in no way hampered by his self-imposed restrictions. Suggestions of lawfulness taken from (2), like all anthropomorphic models, may or may not be helpful to a young science.

In the bulk of the book Köhler argues the alleged superiority of "dynamics" over "machine theories." To say something on this issue, it will be best for me to begin *systematically*. The following three statements circumscribe the logic of what Fechner called outer and inner psychophysics.

1. Assume $(S_1, S_2, \ldots, D_1, D_2, \ldots)$ to be a set of characters, including relational ones, of a physical stimulus configuration P, e.g., the areas (S) and the mutual distances (D) of a pattern of spots. We know that not only the "perceptual" response R_P but also any of the "elementary" responses, R_{S_i} or R_{D_j}, made to the constituents of P will, in principle, depend not only on S_i or D_j

but on the total stimulus pattern P, i.e., on $(S_1, S_2, \ldots, D_1, D_2 \ldots .)$. Some of these dependencies are, very probably, the result of past experience; some others are, equally probably, unlearned.

2. Imagine for a moment the central nervous system to be a sphere, S. The organism's responses (as carried by efferent conduction) are then a function of (a) the total stimulation C entering S at the central endings of the afferent nerves and (b) the state of S itself. Adequate characterization of C includes the spatio-temporal pattern of the fibers discharging into S. Recognition of this latter fact does not make one a nativist in the sense of the classical issue.

3. There is no reason to believe that C, or what is caused by it "inside" S is, in any sense similar to the external stimulus pattern P which is the (partial) cause of C. All that is required is that, as one usually puts it, C possesses the multiplicity necessary to account for the perceptual response to P.

Historically, the point of (3) has never been fully grasped by the Gestalters. What interfered was the speculative doctrine of isomorphism according to which external field, brain field, and phenomenal field all have "the same structure." The historian has only to recall that this supposed identity of structure is also believed to contain the answer to all epistemological problems, and he will recognize it for what it is: a curiously disguised remnant of idealistic tender-mindedness. There is nothing specifically Gestaltish about (1) and (2); the whole frame of reference was in fact established by such "elementarists" as Helmholtz, Kries, and Koenig. Their argument with Köhler's intellectual ancestor, Hering, as far as it is of any systematic interest at all, centers around the isomorphism issue, the allegedly necessary similarity between the phenomenal and the physiological. Nor did the Wundtians need any lessons on the subject. Their relative neglect of it was not due to either ignorance or confusion but to their different research interest, centering on the discovery of relationships among phenomenal givennesses and those classes of "elements" into which they can be "analyzed" under a certain set. This research interest has since proved sterile; not because, as the Gestalters believe, it was unduly elementaristic, but because it looked for syndromatic R-R rather than for "dynamic" S-R relations. The Gestalters' own contribution consists exclusively in the discovery of some S-R laws of the type described in (1); and everybody familiar with the history of 19th century psychology knows that it is by far not as original as one would have us believe. Systematically, nothing is or ever was new except the claim that Gestalt theory and it alone is compatible with the frame of reference outlined in (1) and (2).

Köhler's *arguments* are those usually offered in support of this unwarranted claim. I shall deal with just one that seems to me both fundamental and characteristic. "In the machine theory," we are told, "any local sensory fact is strictly determined by its stimulus. Consequently, the characteristics of stimuli in their relations to one another can play no part in the determination of local sensory experience" (p. 121). By way of *criticism* I would say that if this means anything at all, it means

that the so-called machine-theorist knows only that stimulus S_1 in isolation (or, rather, under standard conditions) elicits response R_{S_1}, and the same about S_2 and R_{S_2}, S_3 and R_{S_3}, and so on. He is, *therefore*, unable to explain that the total stimulus object P, of which S_1 is a part, produces, under the "elementaristic" set directed toward S_1, the response R'_{S_1} which is different from R_{S_1}, and, under another set, the "perceptual" response R_P which is not the "sum" of the responses R_{S_i}. The "therefore" which I have underlined represents the familiar and often exposed *non sequitur*. There are no reasons why we should not find empirical laws—in fact, the behaviorists have found plenty of them—that determine R'_{S_1} as well as R_P as functions of $(S_1, S_2, \ldots, D_1, D_2, \ldots)$ and other factors. And what, if not the discovery and theoretical organization of such empirical laws, is the business of the science of psychology? The very idea of some sort of phenomenal deduction of the "natures" of R'_{S_1} or R_P from those of the R_{S_i} rests on a misunderstanding of the function of science. Yet it seems that this is what the Gestalters themselves try to do and blame the rest of us for failing to accomplish.

As far as those other factors that I just mentioned are concerned, it is at present largely a matter of speculation whether to "place" any one of them into the central nervous system, the receptor organ, or, perhaps, into both. Such physiological speculation plays a tremendous rôle in Köhler's thought. To the rest of us it seems, *qua* speculation, rather sterile; certainly it is not, as claimed, a matter of major systematic import. To return to the main issue, there is, of course, interaction among the variables, in the sense that an organism in a certain state O in conjunction with the stimulus object S alone forms a system that is different from the system composed by the organism in the same state O and the stimulus object P of which S_1 is a part. Do not always, "even" in machine theory, different systems (and different initial conditions) produce different effects? What, then, is there that I can't understand in the fact that the responses in the two cases are different? What is there the behaviorist—or, to pronounce the terrible word, the learning theorist—cannot explain? The problem I have selected shows also the hollowness of the Gestaltist formula that the whole determines the character of the part. S_1 is always the same, whether it occurs in isolation or as a part of and related to the other parts in the whole P. What may or may not, according to circumstances, be different are the responses, R_{S_1} and R'_{S_1}, that are made to it. But such logic chopping is, perhaps, too positivistic for the visionaries of the whole.

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BARTLEY, H. S., & CHUTE, ELOISE. *Fatigue and impairment in man.* New York: McGraw-Hill, 1947. Pp. ix+429.

The authors are to be congratulated upon a scholarly and systematic

presentation of the data generally subsumed under the heading of fatigue. They have indicated the inadequacies in our present knowledge and have suggested general methods and procedures, both logical and experimental, for future research. Lack of delineation of terms and confusion in fundamental concepts and techniques characterize the present status of the field. The opening chapters summarize the various views of psychologists and physiologists on fatigue. Diversified study has given rise to unrelated kinds of "fatigue." Fatigue has been identified with physiological and chemical changes, with work decrement, with feelings of tiredness.

The authors propose a single over-all formulation based upon the assumption that "fatigue is real and identifiable" and that "it is the expression of the whole person." The unity of the individual and the uniqueness of personal development demand a "science of the person" in which organization and continuity are essential. Precise formulations of basic concepts are necessary. Fatigue, according to the authors, is a complex experience including feelings of limpness and bodily discomfort, arising in a conflict situation. The general alignment of the individual may be described as aversion. Impairment is distinct from fatigue. It is a "physiological change in tissue which reduces its ability to participate in the larger aspects of organic functioning." Only the methods of physiology and biochemistry can identify impairment. Work output can give little clue to what is happening within the organism.

Following this general orientation, a survey of the work done on fatigue and impairment is presented. The studies reported include "(1) work on localized tissue function, both in vitro and in vivo; (2) studies of work output and its decrement; (3) studies of physiological function under limiting conditions such as extreme heat and cold and water, oxygen, and sugar lacks; (4) work on deterioration in quality of performance, as in perceptual anomalies, motor anomalies, anomalies of speech, etc.; (5) studies of changes in specific physiological functions considered as indices of more general changes; and (6) clinical work on syndromes such as chronic fatigue."

One might take issue with the statement that "fatigue is an unpleasant experience" (p. 1). After playing tennis for several hours, one may have feelings of limpness, bodily discomfort, and an aversion for continuing the game; but this is certainly not an unpleasant experience. One may object to limiting fatigue to experience. If, as the authors contend, fatigue is an expression of the whole person, then it should include bodily changes and overt behavior as well as the "subjective" concomitants. The experiential aspects may be designated as "feelings of tiredness." Less confusion would result in interpreting existing data if this terminology were to be adopted.

The methods of research proposed by the authors are vague. Recog-

nition is given to the emergence of new qualities under changing biological conditions. Fatigue can be measured only in terms of several correlated phenomena. The authors indicate that the methods worked out by Angyal embody the essentials for a "science of the person" and offer a background for a study of fatigue. However, any method proposed should take into consideration compensatory mechanisms arising to delay the onset of fatigue. Pre-experimental assumptions of immediate effects should be replaced by long-time studies and by a recognition of delayed effects. The authors object to any differentiation of fatigue, and since "fatigue is experiential," all fatigue is assumed to be "mental." This does not necessarily mean that there are no different qualities or patterns of fatigue. More precise introspectional reports may reveal patterns which correlate significantly with objective records.

Much of the material in the book is necessarily technical. The hope of the authors that the book would be of interest to individuals not particularly versed in the special fields involved would be better realized if a glossary of technical terms had been included. Readers will be grateful that footnote references covering over 500 sources and an author index are provided. A topical list of visual aids at the end of the text may be helpful to instructors dealing with fatigue topics.

This book should be a guide and a reference for all research on fatigue. The authors have integrated a mass of disorganized data, suggested problems for future research, and revealed basic weaknesses and contradictions in the existing knowledge on fatigue.

PHILIP WORCHEL.

University of Texas.

CARMICHAEL, LEONARD, & DEARBORN, WALTER F. *Reading and visual fatigue.* Boston: Houghton Mifflin, 1947. Pp. xiv+483.

The authors have written a book which will undoubtedly be of interest and value to psychologists and all others concerned with obtaining a knowledge of reading as a task. The book is much more a treatise on a number of processes incidental to or involved in reading, than one on the subject of fatigue itself. Certain points of view regarding fatigue are reviewed not only in the first chapter carrying the title, "What is Visual Fatigue?" but elsewhere throughout the book. Nowhere do the authors come to any really definitive conclusion as to what fatigue is. While recognizing that during, and as a result of, activity manifold changes occur within the organism, they utilize "work output" as the indicator of fatigue, ignoring subjective factors. Apparently they succeeded in establishing task conditions within which the subjective factor was not a problem as far as they were concerned.

They found that six consecutive hours of reading fiction or nonfiction in the form of book print or projected microfilm did not lead to work decrement, impairment, signs of nervousness or permanent

changes in eye movement patterns. From this they conclude that there were no signs of fatigue at all. They did find mild expressions of discomfort and the willingness to quit, and certain feelings of stiffness, etc. As the reviewer has already implied, the authors succeeded by one means or another in taking care of the subjects' motivation so that little impatience or dislike for the task was involved. This has very often been the case in studies of fatigue. The level of monetary rewards or the interest in the task itself is made to preclude the development of distaste, discomfort and other untoward effects.

The lack of such effects may have accounted more largely for the kind of visual performances found during the six-hour task span than was recognized. The authors point out that the most direct way to study fatigue in reading is to prolong the task of reading, hence by their use of a six-hour period they feel that they established the conditions for the appearance of fatigue, if a reasonable amount of such activity will in fact induce it.

Chapter 2 contains a good discussion of the eye movements involved in reading. In about the same way, Chapter 3 is a review of the literature on the act of blinking and how blink rate is used as an indicator of physiological condition. Chapters 4 and 5 deal with the subjects of line width, type size, form, etc., and with the relation of the level of illumination to reading behavior. Other chapters deal with recording eye movements by electrical methods, and various other measures of performance during reading, including those used in the authors' own studies. The behavior of the subjects during the reading of microfilm and book print are compared. No differences were found.

The volume contains extensive appendices which report the data of the authors' experiments. The book is well written and is a useful reference book regarding overt and measurable features of reading performance and is without competition in this field.

S. HOWARD BARTLEY.

Michigan State College.

HALL, V. E., CRISMON, J. M., & GIESE, A. C. (Eds.) *Annual review of physiology* (Vol. IX). Stanford Univ. P.O.: Annual Reviews, Inc. & American Physiological Society, 1947. Pp. vii+736.

Volume IX of the *Annual Review of Physiology* contains 27 chapters each dealing with some particular field or topic. Although there is no general review of physiological psychology in this volume, a psychological topic, *The experimental neurosis*, is covered by Liddell. The 12-page review is based on 26 references appearing between 1941 and 1946. The data on the rat are dismissed with reference to Finger's recent article in the *Psychological Review*. Techniques for producing experimental neuroses are appraised and critical discussions are presented concerning

the relationship between experimental neuroses in subhuman animals and neuroses and psychoses in man.

A large part of the material included in "Physiological Effects of Heat and Cold," "The Respiratory System" and "Exercise" concern data on the total organism (often man) and are as appropriate to psychology as to physiology. The chapter on "Electrical Activity of the Brain" will be useful to all psychologists directly or indirectly interested in EEG measurements.

The chapter, "Special Sense, Cutaneous Sensation," is brief but critical and covers a five-year period. An authoritative review, "The Somatic Functions of the Central Nervous System" is presented. Data concerning the cortical localization of taste may be of particular interest to psychologists.

Many of the other reviews will be of value to specialists in various fields of psychology. *Physiological Reviews* itself forms one of the useful reference journals in this field and, as such, is an aid and guide to both graduate students and faculty engaging in bibliographic research. At present, *Annual Reviews* are published for the fields of biochemistry, physiology and microbiology, and they have proved to be most useful to students both in the named fields and in related fields. An *Annual Review of Psychology* should prove equally useful and the forthcoming addition of such a title to this series merits the active support of psychologists.

HARRY F. HARLOW.

University of Wisconsin.

WOLFF, WERNER. *What is psychology? A basic survey.* New York: Grune & Stratton, 1947. Pp. xiii+410. $4.00.

This volume is a novel effort to introduce the average liberal arts undergraduate to the scientific study of human nature by insisting that "psychology works with opposites, that it deals with different points of view simultaneously, and that there are many answers to any one question in this science of contradictions." The usual topics of an elementary text are presented in terms of a distinctive combination of Gestalt and psychoanalytical, i.e., so-called "depth," concepts plus illustrative material drawn from unpublished amateur student investigations, some downright clever.

Wolff's position—which must be quite disconcerting to beginners with their cultural roots in American empiricism—is that psychology does not deal with definite answers. Since it is the science of the inner man, it is the science of contradictions (but not a contradictory science?); "the truth lies in the very differences, in the dynamics." To the core question: Shall psychology help to develop the Robot or Man? there is little doubt which *definite* response the author would give.

Wolff's familiarity with German literary and scientific materials appears to good advantage in imparting a valuable scholarly and "universal" flavor to many of his discussions, but this asset must make it difficult for even the best Bard sophomore to verify his improbable claim that "the case of the Swiss poet, Conrad Ferdinand Meyer, demonstrates that a man up to his thirty-ninth year might show traits of mental deficiency and suddenly, from then to his sixty-seventh year, unfold his genius" (p. 215). His chapter on Motivation is disappointing and would probably have been strengthened had his significant conclusion that "motivation is the great force that unifies the different manifestations of psychological activity" (p. 247) been better documented or even used as the theme about which to build his discussion. The personality materials, on the other hand, are developed with exceptional insight and skill, and display the author at his professional best.

This impression of pronounced unevenness of performance recurs repeatedly as the various sections succeed each other. The concluding chapter, e.g., is a brilliant piece of philosophizing which all mature psychologists could profitably consider. Wolff plausibly insists that psychology is a "bridging science" or relational system which by its very nature cannot be defined by one static term but is the most dynamic and "manifold" of the sciences. In line with some recent efforts to make values more central in general field practice, he holds that the goal of psychology is not only the acknowledgment but also the changing of present conditions, i.e., an "imperative psychology" is as legitimate and scholarly as "comparative psychology."

The treatment of most issues and the general approach is probably too personalized to meet the demands of those instructors who prefer a standard and "representative" text; but for those who find classroom stimulation in acknowledged pioneer departures from the pedagogical norm, this paradoxically systematic yet casual work has much to recommend it.

GEORGE W. HARTMANN.

Teachers College, Columbia University.

GEMELLI, A., & ZUNINI, G. *Introduzione alla Psicologia* (Introduction to Psychology). Pubblicazioni dell' Università Cattolica del Sacro Cuore, Nuova Serie, Vol. XX. Milano società Editrice "Vita e Pensiero," 1947. Pp. xv+438.

This *Introduction to Psychology* is a book very different from the typical American text which might bear the same title. It is stated in the preface that an adequate knowledge of the literature of the subject is presupposed on the part of the reader, and the approach is a highly systematic one.

The book opens with a brief history of modern psychology. This is

followed by a discussion of the dualism of psychology, its subjective and objective data, and its relation to biology. Only after these matters have been thoroughly treated do the authors go on to the consideration of such special processes as perception, memory, affection, intelligence and will. After a thorough treatment of these functions, they devote a chapter to the psychological point of view in the study of language. They then turn to a consideration of behavior, under the headings of instinct, intelligent behavior of animals, human behavior, and social behavior. The book closes with a discussion of personality and the problems of characterology.

The point of view of the authors is that characteristic of Thomistic psychology generally. They regard psychology as the study of man, or of psychic activity, which is the essence of the life of man. These authors, however, are much more concerned than Thomists usually are with mental functions and their biological basis. Perhaps just because they are so well aware of their own view-point, and of the assumptions on which they proceed, the authors are highly receptive to the contributions of other schools. They are thorough in their examination of divergent points of view and try to reconcile them where they find that they logically can. Unfortunately the volume is marred by a number of typographical errors and minor inaccuracies. These, however, are of small significance in comparison with the range and depth of the work as a whole.

JOHN T. METCALF.

University of Vermont.

WILLIAMS, ROBIN M. *The reduction of intergroup tensions: A survey of research on problems of ethnic, racial, and religious group relations.* Social Science Research Council Bulletin 57. New York: Social Science Research Council, 1947. Pp. xi+153.

There has been considerable discussion of the nature and extent of intergroup tensions in this country, and of the manner in which they have been affected by recent events. There can be no argument, however, as to the seriousness of the situation which they represent, and the importance of doing something about this basic threat to our democracy. The fact that many groups are active in this field—there were 123 national organizations listed in 1945 as dealing with race relations—is in itself encouraging, but would obviously be much more so if we had specific information that the techniques used are effective. Much research in this area is in progress, but there can be no doubt that more is needed. With this in mind, Williams writes that "The immediate objectives of this memorandum are to examine research needs and opportunities involved in the problems of reducing tensions among ethnic, racial, and religious groups, especially in the United States; to raise a limited number of questions which warrant intensified research effort;

and to suggest some possibilities for fruitful testing of the more important hypotheses" (p. 1). It would be difficult to find more important research objectives in the social sciences today.

It is not easy to give an adequate summary of the contents of this stimulating memorandum. There is an appraisal of existing programs, including a review of representative studies of the "before-and-after" variety, with emphasis on the deficiencies in existing research materials. This is followed by a series of propositions relating to the origins and prevalence of hostility, types of hostility and conflict, factors in the incidence of hostility and conflict, reactions of minority groups, and approaches for the reduction or control of hostility and conflict. The large majority of these propositions have a high degree of plausibility, even though they cannot usually be regarded as completely demonstrated in the present state of our knowledge. The next section presents selected possibilities for research; historical, comparative and genetic studies; research on the effects of information, education, and propaganda; on the effect of personal contacts; on the effects of legal enactments, law-enforcement activities, and political "pressure" tactics; and on social organizations and programs for reducing general tension. Several illustrative projects—for example, on methods of integrating Negroes into industrial plants, on the effects of recreational opportunities upon intergroup hostility, on the effects of an educational program, etc.—are presented in a more detailed study design. There is rich material here for the research-minded, and the memorandum as a whole is "must" reading both for experts in this field, and for those hoping to break into it.

Many readers will, however, be somewhat disturbed by the organization of the material, even though they are almost certain to find the content rich and suggestive. The division between propositions on the one hand, and possibilities for research on the other, seemed to this reader, at least, somewhat cumbersome and artificial. The research and the propositions are in fact so closely integrated and interwoven, that the separate listings frequently confuse more than they clarify. For example, proposition No. 101 states that "a vulnerable minority can itself help to reduce hostility" (p. 77) by certain types of behavior. "Possibility for research" No. 22 deals with "majority group reactions to changes in minority behavior" (p. 88). It would certainly seem more logical to have these presented within the same framework, rather than in two distinct sections of the memorandum. Many similar examples could be given.

In the second place, the long list of propositions—102 in all, not including the sub-hypotheses—differ widely in importance, and it would have been helpful to many potential investigators to have Williams' expert judgment as to the areas in which research is most urgently needed. Some of the propositions, such as those dealing with the effects

of intergroup contact and collaboration of various sorts, refer to concrete, practical techniques which have immediate and direct application to the problem of reducing tensions; proposition 5, "Infants and preschool children typically do not exhibit prejudice toward ethnic or racial groups. Prejudice is learned." (p. 52) appears to be so certain, that more research in this area is hardly necessary—or at least is not now needed to the same extent as in the case of many others.

Some of the definitions which Williams gives also seem to require further clarification. Prejudice, for example, is used in the sense of "a negative attitude which violates some important norms or values nominally accepted in the culture" (p. 37). Does that mean that if the culture of Germany under the Nazis included Anti-Semitism, without violating the accepted norms, it would thereby cease to be prejudice? Ethnic group is defined as "one possessing continuity through biological descent whose members share a distinctive social and cultural tradition" (p. 42) and is distinguished from racial and religious groups. This is one use of the term, but in the literature it has frequently been extended to include racial and religious groupings as well. These are minor points, perhaps, especially since Williams recognizes his definitions as tentative, but it might have been worth while to relate his own formulations to others in common usage.

This memorandum was prepared under the direction of the S.S.R.C. Committee on Techniques for Reducing Group Hostility, consisting of Leonard S. Cottrell, Jr., of Cornell as Chairman, Charles Dollard of the Carnegie Corporation, and Carl Hovland of Yale. They and Robin Williams have performed an exceedingly valuable service.

OTTO KLINEBERG.

Columbia University.

TERMAN, L. M., & ODEN, M. H. *The gifted-child grows up.* Stanford, Calif.: Stanford Univ. Press, 1947. Pp. xiv+448.

Representing the fourth volume in the series entitled *Genetic Studies of Genius*, this book is primarily a report of the follow-up data obtained in 1940 and in 1945 on the original California group of gifted children. By 1945, the group had reached an average age of 35 years, an age at which adult careers are clearly taking shape. The earlier of these two follow-ups included the administration of numerous psychological tests as well as personal interviews and observations by field investigators. The 1945 follow-up was a less intensive one, conducted entirely by mail. The initial investigation covered a total of 1,528 California school children, ranging in I.Q. from 135 to 200 and in age from 3 to 19. By 1940, 61 of the subjects were deceased and 33 could not be traced, leaving a total of 1,434 on whom the major data of the present analysis are based. The results of the 1940 survey, when the average age was slightly under 30, are supplemented throughout with corresponding data from

the 1945 mail follow-up. The first six chapters of the book provide a good over-all view of the earlier stages of the project, including a summary of the original characteristics of the gifted children, as well as the major results of the six-year follow-up reported in Volume III. The present book is thus a complete survey of the first 25 years of the project and can be read without reference to the earlier volumes.

The most conspicuous finding in the successive follow-ups is that the group as a whole retained its superior status in all respects. Mortality rate was below that of the general population; physical and mental health remained superior; educational and occupational achievement were consistently outstanding. Adult intellectual status was determined by a specially designed *Concept Mastery Test* consisting of synonym-antonym and analogies items and covering a wide variety of fields. Although requiring only about one-half hour, this test was shown to have high reliability and validity for superior adult groups. Through this test it was possible to estimate that the mean I.Q. of the gifted group in adulthood was about 134, representing a drop of 17 points from their childhood average. The authors point out that such a drop is no greater than would be expected from regression, as a result of (1) errors of measurement in the Stanford-Binet and Concept Mastery tests, (2) differences in the functions sampled by the two tests, and (3) maturational and environmental changes.

Educationally, the gifted group excelled in all comparisons. They attended college in much larger numbers, took graduate degrees much more often, and received better grades and many more academic honors than any other groups with which comparisons were made. For example, the percentage receiving Ph.D. degrees was over five times as large for the men and over eight times as large for the women in this group as in a representative sampling of college graduates. A special study of educational acceleration in the gifted group not only showed acceleration to have been common, but lent no support to the view that such acceleration may be detrimental. Any slight social handicap suffered by the very accelerated subjects during adolescence seems to have been fully overcome in later years. In fact, whatever differences were found in later achievement and adjustment tended to favor the accelerated group.

In occupational level, the gifted group stood far above the average, being represented in the higher professions by eight or nine times its proportional share. The incidence of delinquency, alcoholism, or serious maladjustments was less than in the general population, and there was considerable evidence of good emotional and social development and breadth of interests. Participation in extra-curricular activities was conspicuous in high school and college; hobbies and avocational interests were well-developed and closely resembled those of any contemporary American group. An active interest in political and social

matters was suggested by the fact that 91% of this group reported voting in all national elections in contrast to only about 70% in the general California population. The social and political attitudes of the group showed no marked deviation from the generality. A special chapter is devoted to the subjects' war record, which appears to be quite disguished and creditable.

Of considerable interest are the data on marital status and marital adjustment. The incidence of marriage among both men and women in the group is above that of college graduates of the same age and about equal to that in the general population. Intelligence tests of the spouses as well as the offspring showed both to be quite superior, but below the average of the gifted group itself. On specially designed tests of marital aptitude and marital happiness the present group was somewhat superior to other groups less highly selected in intelligence. Sexual adjustment was in all respects as normal as in less gifted groups. Divorce rate was no higher than in the generality of comparable age.

A special study of individuals whose initial I.Q.'s had been 170 or higher shows them to compare favorably with the rest of the group: they were more often accelerated in school, received better grades, and continued their education longer; they were as well adjusted emotionally and more successful vocationally than the rest of the group. In another special study, on subjects of Jewish descent, no significant difference from the rest of the group was found in ability or personality traits, except that the Jewish subjects showed a somewhat stronger drive to achieve, continued their education longer, were more successful vocationally, and had a somewhat lower divorce rate. Probably one of the most interesting analyses in the entire survey is the comparison of the 150 men rated most successful (*A*) with the 150 rated least successful (*C*) in adult achievement. Despite the high average accomplishments of the entire group, the adult achievement of the "gifted" subjects ranged from "international eminence to semiskilled labor" (p. 311). In the effort to throw some light on the correlates of adult achievement, the two contrasted groups within the present sampling were compared in about 200 items of information secured between 1921 and 1941. The most conspicuous differences were the superior educational and vocational level of the parents of the "*A*" men, as well as the subjects' greater "drive to achievement."

On the whole, the book is very well organized and the voluminous data have been summarized clearly and concisely. The presentation might perhaps have been improved by a more complete integration of the 1940 and 1945 data in certain chapters. Similarly, the chapter prepared by Quinn McNemar on the results of the *Concept Mastery Test* (Ch. XII) could have been effectively combined with the preceding chapter, which discusses much the same data in a different way. The reader may be somewhat confused by these two chapters. Throughout

the book, the authors give relatively little interpretation, adhering closely to a summary presentation of the quantitative findings. Their caution in this regard is of course highly commendable, but perhaps a little more liberal sprinkling of tentative hypotheses, clearly presented as such, might have enhanced the value of the report. It might also be noted that participation in the study may itself have affected the subsequent careers of the gifted subjects. Had a control group been followed up over the entire period and in exactly the same way, perhaps the extent of this effect could have been at least partially gauged.

To any psychological reader, however, the monumental contribution of the study itself overshadows any other impression which the book may leave. In size of sampling, carefulness of planning and execution, extent of data, and sheer duration of follow-up, this study obviously stands alone in psychological research. The authors refer to the present stage as the midway point in a proposed 50-year follow-up, which is to include periodic testing of the original subjects and their offspring. Besides the direct information which it provides on the origins and dynamics of "genius" in our contemporary culture, this project has already stimulated a number of divergent studies and has led to the construction of several widely used psychological tests. These by-products represent highly significant contributions in their own right.

ANNE ANASTASI.

Fordham University.

COLE, LUELLA, & MORGAN, JOHN J. B. *Psychology of childhood and adolescence.* New York: Rinehart, 1947. Pp. xi+416.

This undergraduate text-book is concerned with "human development from early childhood to the later adolescent years." Although originally planned as a joint project by the two psychologists, it was carried through wholly by the senior author due to the death of Dr. Morgan.

In keeping with the increasing emphasis upon the genetic approach to human behavior, Morgan's *Child Psychology* and Cole's *Psychology of Adolescence* were fused into the present book. This procedure has required selection and considerable condensation of the material of these texts. However, the loss of detail has been compensated for by greater continuity of the phases of human behavior under scrutiny.

Physical and motor development, emotional and social growth, motivation, play and interests, intelligence, language, attitudes, personality, and homes for children and adolescents are the principal subjects covered in the twelve chapters. Each of these facets of human behavior is treated developmentally in so far as research data will permit. The style is descriptive, with frequent footnotes to references of original sources to support the facts, interpretations and generalizations presented. Graphs, charts, tables, pictures, and diagrams are liberally

used to emphasize and clarify the various points under discussion. A list of selected readings at the end of the book highlights even further certain topics.

The genetic approach emphasizes the importance of certain modifying factors in the developmental process. One such influence is the home, but little research is available to document this chapter in the text. While a crude index, it may be noted that only eight references support this chapter, while such topics as social growth, intelligence, play and interests have four to six times as much documentation. Although this unevenness is hardly the responsibility of the authors of a textbook, it does show the need for further research in the neglected areas. This observation gains in importance since this is a book designed primarily for prospective teachers and others who are not likely to become research workers themselves, but who need a scientific basis for their various practical problems.

Some attention to the work that is being done on the analysis of human development in terms of the differential patterns of values according to social class would have added to the general presentation and to the chapters on motivation and social growth in particular. Such statements as, "There is no attempt in Negro homes to train a child to follow a set rhythmic pattern; instead, the whole atmosphere is one of freedom to sing and dance as he pleases" (p. 57); or "Inclusion or exclusion [in or from an adolescent crowd] is settled upon the individual merits of each boy or girl. It is not a matter of family alliances, nor exclusively a matter of similar social backgrounds" (p. 176); and the general underlying assumption of middle-class values and point of view —would all certainly be modified by consideration of the research and theory pertaining to social classes.

Psychology of Childhood and Adolescence will be found useful as an introduction to the developmental aspects of human behavior, provided its general orientation and the need of further basic research in this field are kept in mind.

S. O. ROBERTS.

Fisk University.

HAMILTON, GORDON. *Psychotherapy in child guidance.* New York: Columbia Univ. Press, 1947. Pp. xiv+340.

The literature on psychotherapy with children is as yet relatively meager and is represented by almost as many different points of view as there are publications. Child analysts, starting with Melanie Klein and Anna Freud, have presented their versions of strictly psychoanalytic therapy. Non-analytic psychiatrists and psychologists have presented other and somewhat differing approaches. This volume presents another point of view, that of the psychiatric social worker. Since psychiatric social work has derived its concepts primarily from psycho-

analysis and its experience from social case work with underprivileged families, Miss Hamilton presents a merging of the two, a "psychosocial" therapy which she sees as a modified form of psychoanalysis. The stated purpose of her book is "to describe adaptations of therapy carried on by social workers at the Jewish Board of Guardians" in New York. Actually, it is written as an authoritative text on the diagnosis and treatment of behavior disorders, and as such presents a substantial contribution to the literature in this field.

In reality, one is struck more by the likenesses than the differences in the various therapeutic approaches to children's problems. All would agree with Miss Hamilton that the therapist-child relationship when skilfully handled is the chief therapeutic medium; that allowing opportunity for expression of usually inhibited impulses, the use of reassurance, interpretation, etc., are simply ways of putting this relationship to work; that there is always a need to understand as much as possible of the stresses and strains in the particular child's environment and to modify these in whatever ways possible (notably, in giving as much if not more concern to the parent's than to the child's anxieties); that the use of play is merely a means of providing the child with a medium which he can accept and use; that, in the last analysis, it is always the patient rather than the therapist who effects the necessary changes in attitude. Since all psychotherapies—consciously or not—have been greatly influenced by psychoanalytic thinking, these likenesses are fundamental to their origin. But one real difference should be mentioned: that each proponent of each school of thought sees his way as *the* way. There is not yet the intercommunication between various disciplines which will lead to enlightenment on basic problems in all therapy rather than to re-statement of essentially the same principles.

The wealth of material covered in this book is both its virtue and its chief defect. In spite of the emphasis on a social casework approach fully five-sixths of the book is devoted to a very rich and full discussion of the dynamics of individual personality development. Four of the 12 chapters are devoted to diagnosis, six to treatment, and one each to an introduction and conclusion which relate specifically to the field of social casework. Diagnosis is conceived in very broad terms because of the admitted difficulty in attaching labels to behavior syndromes with any degree of accuracy. Miss Hamilton uses three general categories: "the child who acts out his impulses" (primary behavior disorder), "the anxious child" (i.e., with internalized conflict) and "the severely disturbed child" (with organic disease, psychosis, severe psychoneurosis). This last chapter is especially helpful in delineating the types of problems which should be handled only by highly experienced workers. The chapters on treatment are divided according to general developmental level: "young children," "the older child," "adolescents,"

"the family." While such a division of necessity cuts across the basic treatment processes which occur at all levels, it has the virtue of forcing the reader to weigh the child's behavior against the development of "normal" anxieties at each stage.

The defect here occurs only in that Miss Hamilton has tried to cover so much in one volume. In her attempt to present the infinite variety and subtlety of all children's behavior, as well as the limitless variations in patterns of individual behavior, her discussions prove confusing at times even for the initiated. It is just this breadth of approach, however, which makes the book of great value to the student of any discipline who is concerned with the emotional problems of childhood.

MARY D. FITE.

State University of Iowa.

HARMS, ERNEST (ed.). *Handbook of child guidance.* New York: Child Care Publications, 1947. Pp. 751.

The two major aims of this handbook are (1) "to set forth in the broadest sense, the theory and practice of child guidance for the normal as well as the subnormal child" and (2) " . . . to outline the broad fundamental problems which the science of child guidance must encompass rather than to indicate the all too apparent deficiencies of our present practice in the field. Its main task is not so much one of specific detail as one of broad definition and outline."

Certainly, the outline has been broadened so as to cover the entire field of child guidance. It is divided into eight main parts:—"The Development of Child Guidance in the U. S. A."; "Guidance of the Normal Child"; "Guidance of the Physically Handicapped Child"; "Guidance of the Problem and Subnormal Child"; "Training for Child Guidance"; "The Social Aspects of Child Guidance"; "Religious Aspects of Guidance"; "Special Viewpoints for Guidance" (which include the theories of Freud, Adler and Jung).

Two types of readers will be interested in this book: parents, and psychologists or teachers who are working in this field. Parents will have need of it, since it answers hundreds of simple questions asked by parents with an overabundance of anxiety and an appalling lack of common sense. Moreover, those working in this field will like the book, since 90 percent of it is concerned with basic principles that are being taught today in child psychology. The remainder of our population will only find it interesting in spots (small spots). As a whole the psychologists in other fields will have the general and correct impression that the book would be greatly improved if the padding were removed, which would reduce its size to about one half. For example, we defeat our purpose in explaining the obvious unless some precision is employed. This is what happens throughout most of "Part Two: Guidance of the Normal Child," with the exception of the topic on the superior child and prodigy,

by Harms, which is treated in a skilful and original way. Again, in dealing with the social aspects of child guidance, the authors include separate and distinct topics for "Camping and Guidance" (Cabot), "Guidance Problems of Negro Youth" (C. S. Johnson) and "Special Guidance Problems with Migrant Children" (L. C. Wagner). There appears to be a shortage of interesting facts for this long span of 47 pages. The problems obviously warrant far more space, but our knowledge of these subjects has serious limitations.

One important chapter deals with the theories of Freud, Adler and Jung. Oddly enough, textbooks on Child Psychology usually go into great detail concerning ancient theories such as that of the 19th century G. Stanley Hall and they completely ignore contemporary theories. The authors of the present chapter (M. Schoenberger, Alexandra Adler and F. Wickes) are to be commended for their clarity and precision.

One should not place too much emphasis on the interest value of a handbook, for after all it is a sort of encyclopedia. One should ask: "Is it correct and is it complete?" I have already suggested that, if anything, the book is too complete. As for the principles and facts, they are truthfully presented, although, as to be expected, one comes across controversial issues. I shall refer to only one of these. Harms in his chapter on "Guidance of the Superior Child and Prodigy" states:

> At a time when one feels himself called in defense of American democratic social ideals against European Nazistic philosophies, he is profoundly impressed to observe how closely the educational conceptions of the American Behaviorists are related to the latter, and how far they are removed from the conceptions of the founders of the American educational traditions . . . .

I thought Behaviorists had already been accused of everything, but this endictment is truly original. Apparently Harms is accusing Behaviorists of stealing little childrens' souls, for he continues:

> All ethical and religious issues consist of a transformation of lower forms of affects and desires into higher moral attitudes. All of these are, however, expressions of will. Moral and ethical problems are always will problems.

I would not embarrass this author by asking him what he means by "*Will.*" As a contemporary Behaviorist (who like all contemporary Behaviorists disbelieves most of what Watson professed) I would say that the normal child has the capacity to be trained in such a way that he comes to be attracted to goals other than the satisfaction of tissue needs (e.g. music, philosophical and theological interests, etc). These acquired needs can become so strong that he will prefer their satisfaction to the satisfaction of the so-called baser needs antagonistic to the so-called higher ones. When this occurs, one may say (if he cares to) "This child has character," or as Mr. Harms would say, "He has a strong will."

LIVINGSTON WELCH.

Hunter College and *Cornell Medical College.*

SNYDER, W. U. (Ed.) *Casebook of non-directive counseling.* Boston: Houghton Mifflin, 1947. Pp. viii+339.

This is a major contribution to the literature on non-directive counseling in two respects: (1) it presents verbatim records of the conferences involved in five cases, with different consultants, and (2) it presents a classification of counselor and client statements. The book will provide illustrative material for persons who are teaching courses in psychotherapy, mental hygiene, abnormal or clinical psychology. It might be helpful in psychotherapy to have clients with similar problems read the case that resembles theirs.

The first case, counseled by W. U. Snyder, appears to involve primarily anxiety and withdrawal tendencies related to a strong egotism and a fear of failure. The second case, counseled by C. R. Rogers, involves extreme withdrawal tendencies based on fear of failure and numerous other factors. The third case, counseled by G. A. Muench, involves a struggle to eliminate extramarital social and sexual experiences. The fourth case, counseled by A. W. Combs, involves a struggle to accept a physical deformity and for self-acceptance. The fifth case, counseled by V. M. Axline, involves primarily adjustment to the client's mother. The majority of the counseling interviews for each of these five cases are reported verbatim—i.e., constitute the major portion of the book.

Five cases do not prove anything, but they do illustrate the non-directive technique in action. Dr. Snyder points out that the final outcome of some of the cases is still in doubt and that it is impossible to determine the respective contribution of the counseling and other factors to the clients' progress.

The cases raise many questions about non-directive procedures. First, two counselors (Snyder and Rogers) give their clients books (Shaffer's *Psychology of Adjustment*, and Travis and Baruch's *Personal Problems of Every-day Life*). This practice appears to the reviewer to be in contradiction with several non-directive principles. Second, the case reports contain several testimonials by the clients for *directive* therapy. For example, Mary Jane Tilden, counseled by Rogers, states (p. 193) that two friends "tell me I'm as bright as other people. Oh, of course, that is flattery, but it has helped me . . . ." Uncontrolled directive therapy is a possible factor in the "success" of non-directive therapy. Third, the research on non-directive techniques to date has not attempted to determine whether statements made by the counselor with the intention of being non-directive are interpreted by the client as directive and are, therefore, *directive in effect.* No counselor can safely assume that his own intentions and the client's interpretation of his statements are identical, yet present non-directive theory appears to make that assumption. The client's reaction may give to the counselor's statements a directive role. This is illustrated most clearly in the case

counseled by Muench. At the close of the therapy, the client praises Dr. Muench for his adherence to his non-directive role but the client also makes these statements: "Your silence, and your shrewd reflections, and your subtle important meanings have gained my utmost confidence. . . . You always came through with some excellent things when they were needed." Fourth, many psychologists will contend that counselors' statements which are classified as "clarification of feeling" are in reality interpretative and directive.

These points and others raise the question as to whether non-directive therapy *in practice* is as "pure" as we have been told it is *in theory.*

The book is a definite contribution to the literature on psychotherapy. Detailed reports on therapeutic processes which use other techniques would be equally useful.

WILBUR S. GREGORY.

University of Redlands.

CAMERON, NORMAN. *The psychology of behavior disorders.* Boston: Houghton Mifflin, 1947. Pp. xxi+622.

The matter in this book is roughly that found in the standard texts in abnormal psychology (with some novel rearrangements and at least one salutary omission). The manner of the book is hailed as "unique" in a very flattering editor's introduction by Leonard Carmichael. The author himself agrees with Carmichael in his preface: "this is a presentation of the neuroses and psychoses from a consistently biosocial point of view. It follows a prediction made five years ago that psychopathology—or *behavior pathology* as I propose to call it—will shift progressively in emphasis, from speculations about a psyche in a somatic container, to the study of the operations of human organisms in a social field." Furthermore, "it differs radically from the contemporary psychosomatic approach to the behavior disorders by breaking completely with the tradition of mind-body dualism." And finally, "The point of view which I have developed in this work differs from classical *behaviorism* in rejecting reflexes, instincts and emotions as building blocks out of which human behavior was supposed to be constructed. It is holistic and analytical rather than atomistic and synthetic. In dealing with biosocial behavior, normal and abnormal, the emphasis is less upon the physiological machinery of the individual than upon communication, learning, role-taking and socially derived self-reactions. The biosocial interpretation departs from traditional *psychobiology* in dispensing entirely with the concept of consciousness and the distinction between mental and non-mental. This concept and this distinction are both residues from the once flourishing systems of psychosomatic dualism. We can neglect them in behavior pathology without missing them."

Consequently, as an adherent of psychological field-theory, I ap-

proached the book in a very receptive mood. The subsequent attitude of rejection in this review must be understood from this standpoint. Cameron, in taking a good step forward by adopting a biosocial field-theoretical approach, is forced to take two steps backward by not making it *biopsychosocial*, and I think finally trips and throws the baby of the psychological ego out with the bathwater of the semantically inadequate previous conceptions of it. The final result is a well written, carefully constructed presentation of abnormal psychology, which, however, is unable to deal with many of psychopathology's central problems at other than a descriptive level.

I do not now feel, and have never felt, that the Freudian methodological conceptions were much more than first approximations. As a matter of fact, I wrote nearly ten years ago, that: "Psychoanalysis often speaks of ego and superego and id in the fashion in which medieval philosophers spoke of the Christian soul. The ego takes on all the properties of a selfish Homunculus who pulls the strings for what the psychoanalysts call ego functions; the superego is similarly thought of as a more or less conscious moral Homunculus controlling superego functions; and the id is an unconscious nasty little Homunculus controlling id functions." I am, however, convinced that these conceptions must be sharpened rather than ignored. While Cameron pays lip service to the stupendous achievements of psychoanalysis, he is afraid of all of its most basic implications. I am sure that as soon as clinicians realize that ego, id, superego, conscious and unconscious, regression, repression, transference, etc., are not descriptions of real entities, but rather abstract concepts to account for easily observable and, in clinical practice, almost daily observed behavioral processes, that they will become better clinicians. Cameron is afraid of these terms as if they were bogeymen, and does not seem to have perceived their true methodological role.

This leads him into many difficulties. Although his chapters which deal with the psychosomatic problems are on the whole good, the dynamic processes behind these are scarcely touched upon. Flanders Dunbar does not appear in the index, and the work of Alexander and French and the Chicago Institute for Psychoanalysis is completely ignored.

Further, in adopting what to him seems a simplified nosology, many of the chief problems of modern psychiatry are simply omitted. The index carries no reference to alcoholism, criminality, delinquency, genius, or extremist political attitudes, although some of these are touched upon in passing.

In laying down the book, one comes to the conclusion that the author feels there is nothing really specific in the etiology of the various disease entities. Every chapter devoted to a specific disease entity carries a section on the biosocial determinants of the same, and to the reviewer these are sometimes hard to distinguish.

This comes undoubtedly from Cameron's distaste for dynamic genotypical concepts. Sometimes he approaches them, but in his attempt to remain both semantically pure and purely biosocial, he doesn't even shake hands with these concepts. As a trained and practicing clinician, however, Cameron is forced to deal with the situations that these concepts subsume. When he does so, he is forced into very clumsy circumlocutions which do much to mar his otherwise very readable, and sometimes rather charming style. Just one example: "Repressing is an habitual adjustive technique which reduces the tensions of need and anxiety by preventing the occurrence of a tension-provoking reaction or by inhibiting its development, in the presence of previously adequate stimulation. Repression is neither a conscious nor an unconscious force."

The reviewer's opinions are not all adverse. There is no long section on symptomatology, which is an omission definitely for the good. Psychopathology is really beyond the curiosity cabinet stage, and I hope this omission will serve as an example for future texts. The case histories are all new and well chosen, and they are given in sufficient detail to be actually helpful to the student. The discussion of the growth and development of the human personality in the early chapters, although descriptive rather than truly dynamic, is excellent. The sections on the increasingly important concept of "role taking" are particularly good.

Undoubtedly this book will have a warm reception, particularly from the many professionals who are not yet able, for one reason or another, to stomach the truly dynamic approach of modern psychoanalysis. This is not an attempt like that of Horney's, and many others, to revise psychoanalysis, but rather to go beyond it. I am sure that we will go beyond the first theoretical approximations of Freud. But I am equally sure that we cannot by-pass them. This is what, in the final analysis, Cameron is attempting to do. Although I feel that I have learned something from Cameron's book, I also feel even surer that the next time I am confronted with a new case, I will want to evaluate the nature of the id, ego, superego conflicts, the possibilities in transference, the depth of the regression, the ego strength, before I go on with it.

J. F. BROWN.

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NIELSON, J. M., & THOMPSON, GEORGE N. *The engrammes of psychiatry.* Springfield, Ill.: Charles C Thomas, 1947. Pp. xix+509.

Many attempts have been made to reduce the data of psychology to physiological terms. While this book represents one such attempt it is by no means the best. Starting with the myopic assumption that "all psychology is cerebral physiology," the authors claim that it is the purpose of the book "to present as far as possible the anatomy and

physiology of human behavior." The loophole in the book, of course, is in the phrase "as far as possible," since at the present time that isn't very far. Still we have a book of some 500 pages, so one can only conclude that the authors have not adhered to their original purpose, otherwise there would be far less material in the book than is the case.

The first chapters deal with what are presumably the fundamentals of human behavior. To give some idea of how inadequately the fundamentals are treated it need only be mentioned that emotions are covered in a chapter of three pages, personality in a chapter of seven pages, intelligence, judgment and wisdom in a chapter of two pages, while our old, but not always welcome, friends the instincts and the will are treated more generously but not in any better fashion.

From a nosological standpoint the book is fair. The symptomatic discussion of psychoneurosis, psychopathic personality and schizophrenia is good. However, this discussion is not reinforced by the presentation of a lucid and unequivocal neurological basis for the symptoms discussed. In fact, the inclusion of the neural engrammes as a basis for psychopathology seems to be almost an afterthought in most of the book.

The sections of the book which deal with etiology, diagnosis, and treatment are sketchy and not always accurate. The absence of experimental literature in these areas is especially noticeable, as it is in the case of such topics as conflict, frustration, and experimental neurosis.

The authors appear to be completely unfamiliar with modern psychology. While it is doubtless true that there are some fundamental neurological, and even more fundamental neuro-chemical processes underlying much simple adaptive behavior, these processes are as yet very little understood. On the other hand there is a good argument that the study of the separate parts of a jig-saw puzzle, for example, will not reveal what the final integrated picture will be. That is precisely where this book falls down. It does not present an integrated approach to human behavior. Instead it seems to be a mere concatenation of part-truths with the hope that in their addition there will emerge an integrated picture of psychiatry. Perhaps the parts are present, but the meaningful integration of the parts is not.

In the reviewer's opinion, the book has not reduced human behavior to cerebral physiology nor has it reduced psychiatry to disordered neural engrammes. There is nothing distinctive in the book to recommend it to psychologists or psychiatrists. Its principal difficulty lies in its inability to differentiate clearly between simple biological adaptations, for some of which there may be demonstrated physiological correlates, and complex biosocial adjustments which have thus far no demonstrated simple physiological basis.

ROBERT P. FISCHER.

Marietta College.

HARTWELL, SAMUEL W. *Practical psychiatry and mental hygiene.* New York & London: McGraw-Hill, 1947. Pp. xvi+439.

This book is intended to serve as an introduction to psychiatry for use by student nurses. Its "four definite goals" are: (1) "to help the student develop a dynamic interest in psychiatry"; (2) "to make psychiatry more easily understood"; (3) "to make the teaching of psychiatry practical"; (4) "to permit flexible use of the text." The last goal is considered to be achieved by the division of the book into four more or less autonomous parts; (1) Introduction (9 pages); (2) Medical Psychology (60 pages); (3) Psychiatry (265 pages); and (4) Mental Hygiene (48 pages). There is a general bibliography of some 200 titles, special chapter references and a glossary of terms.

The major section of the volume covers the field of descriptive psychiatry comprehensively and well. The major syndromes are described fluently, in adequate detail and in decent style. It is evident that the author "known whereof he speaks." The other parts of the book are definitely weaker and are marred by numerous minor errors. The discussion of "medical psychology" is banal, superficial, inaccurate and dated. There is a brief chapter on "Schools of Academic Psychology" (Structuralism, Functionalism, etc.), the exact value of which to a student nurse would seem to be, to say the least, debatable. In this book, with a 1947 publication date, behaviorism is described as *still* being the "bad boy" of psychology.

In the chapter on Mental Deficiency, a novel explanation of the usual range of IQ's for a given intelligence classification (e.g., Dull Average 80–89) is presented. "The following table of intelligence quotients is offered as a guide to the categorical levels of intelligence. Since a good social adjustment and peace of mind enable one to use what intelligence he has, two columns of figures are given: the first column may be considered in terms of the unhappy, poorly adjusted person, the second column for the happy, well-adjusted individual." It is exciting to learn that emotional and social adjustment may be quantified in terms of IQ points—10 IQ points, to be exact.

In summary, despite the excellent major section on descriptive psychiatry, the book hardly reaches its stated "definite goals." It is not particularly dynamic, it is not particularly practical and it suffers from "boners" and deficiencies. In justice to it, we should add that it is not worse than many another current text of this type.

ARTHUR L. BENTON.

University of Louisville School of Medicine.

TREDGOLD, A. F. *A text-book of mental deficiency (amentia).* (7th Ed. Rev.) Baltimore: Williams & Wilkins, 1947. Pp. xvi+534.

This familiar book now reappears in its seventh edition; since 1908 it has been a source of information for all those concerned with the

various aspects of mental deficiency. It is a compendium including not only discussion relating to etiology and diagnosis, but also an explanation of the English legal concept and educational provisions, and a presentation of sociological problems. Some psychological tests are provided. There are many photographs of persons illustrating various types of physical abnormalities which are accompanied by defective mental development. Although much of the material is presented in a thoroughly objective manner, nevertheless the book as a whole is somewhat personal and informal, the author introducing interesting comments and experiences as a teacher talking with his students.

The American reader who is not particularly interested in the English Educational Act, 1944, will find the present edition very similar to the sixth edition of 1937. The chapter headings are the same, and there are only a few additions of diagnostic import. These include a short paragraph on the Rh. blood factor incompatibility as a cause of damage to the brain cells, a description of the Laurence-Moon-Biedl Syndrome and of Rud's Syndrome. For the first time there is some discussion of the Psychopathic Personality under that designation; it is accompanied by a long illustrative account of a sadistic murderer who was tried in the criminal courts of London.

Notably missing in this volume is mention of some of the procedures which are marking an advance both in diagnosis and in treatment. Although Dr. Tredgold continually relates mental deficiency to cerebral abnormalities, he says nothing of PEG nor of EEG, methods which are used so extensively for observing abnormalities in brain structure and in brain function in living persons; information thus obtained serves to clarify diagnosis. Also omitted is reference to the newer surgical methods of treating hydrocephalus, and the advances made in medication to control epilepsy. There is no mention of the promising research with glutamic acid for promoting mental efficiency in so ne defective children, nor of blood transfusions as being helpful in decreasing the brain damage ascribable to Rh. factor incompatibility. While it may be that these procedures are confined principally to this country, or that the omissions are due to the lag with which publication follows manuscripts, nevertheless a discussion of these more hopeful frontiers of scientific advance would have contributed much to the knowledge and attitude of the student of mental deficiency.

LOUISE R. HEWSON.

Neurological Institute, New York.

MARKS, ROBERT W. *The story of hypnotism.* New York: Prentice-Hall, 1947. Pp. viii+246.

The purposes of this book are stated in the foreword to be: "to survey some forms of human behavior in the light of what is known of hypnotism and trance states, and to attempt to apply to them a degree of

measurement and predictability." These aims are not fulfilled. The book is an account such as a tyro would give of a subject which experts find it difficult to write about. It is a forced, sometimes flamboyant and more often smart-alec, presentation, compounded of true, doubtful, fatuous and false material.

The first half of the book undertakes to popularize the lore of hypnosis (history, stages, dangers, uses, theories) in the course of which undiscriminate reference is made to James, Janet, Bernheim, W. R. Wells, Hull, Husband, Salter, Erickson, Polgar, Brenman, Freud, among others. The author then explains in terms of hypnosis such varied phenomena as crowd behavior, faith, faith cures, mysticism, ecstasy, spiritualism, religious excesses, and psychological epidemics (general and particular).

What, then, is hypnosis? It is "heightened suggestibility" (pp. 33); "a form of conditioning, a triggering of predispositions" (p. 105); either "Father hypnotism" or "Mother hypnotism" (p. 107), with strong sexual components on the part of the hypnotist (p. 119). The latter is also said to fear the subject, since "There is something weird, uncanny, unbelievable about the seance. . . . The hypnotist believes, perhaps not without foundation, that if he does not proceed with sufficient caution, the subject will get up and punch him in the nose" (p. 117). This mysterious and potentially assaultive hypnosis is desired by " nany fear-ridden psychiatrists (who) would like to treat hypnotized patients because under normal conditions the patients terrify them" (p. 118).

Hard as it is to believe, in view of the above quotations, much of the book deals with hypnosis about as psychological laboratories and psychiatric clinics have shown it to be; but it is a second-hand account and not a sound account at that.

PAUL C. YOUNG.

Southern Methodist University.

FREDERICK, R. W., KITCHEN, P. C., & MCELWEE, AGNES R. *A guide to college study.* New York: Appleton-Century, 1947. Pp. viii+341.

ROBINSON, F. P. *Effective study.* New York: Harper, 1946. Pp. ix+262.

Our colleges and universities are virtually bursting at the seams as the result of swollen post war enrollments. Classes are larger than usual and are filled with a mixture of eager young people of normal college age and much older war veterans who have been out of school for varying periods of time. Many of these individuals are having great difficulty adjusting themselves to the complex educational situations which face them. How to study effectively seems to be one of the major stumbling blocks so far as great numbers are concerned. Because of this situation the writer is very much pleased to note the appearance of two new books in the field of "effective study."

The first of these books *A Guide to College Study* by Frederick,

Kitchen and McElwee consists of thirty-two chapters which deal with six major topics: (1) general principles of effective study, (2) how to read various types of material, (3) how to observe and record, (4) how to write and speak, (5) how to think, and (6) how to prepare for examinations. The book is written in lucid and simple language and can easily be understood by college freshmen. It is splendidly organized and contains a wealth of useful information for the student. Furthermore, the suggestions and procedures which are set forth seem to embody sound psychological principles of learning. For example, the book does not tell the student to concentrate, as was too commonly done by older books on how to study, but rather explains that concentration is a function of interest and that the student must find some interest in what he is doing or he will not be able to concentrate (pp. 24–27).

If the book possesses any weakness, it probably would be that the exercises at the ends of some of the chapters are frequently very brief and do not call for the extended types of activities which would aid one to develop the skills described in the chapters. For example, at the end of chapter 10, the exercises consist of four short questions which the student can answer with little or no effort. The fact that he can answer them indicates that he has acquired some interesting information but gives no assurance that his study habits have been improved. Undoubtedly the authors had in mind that instructors who use this book would greatly supplement the exercise material. The book on the whole is amazingly complete, and probably would rate as one of the most readable books of its kind that has been published.

Robinson's *Effective Study* is a revision of the author's earlier *Diagnostic and Remedial Techniques for Effective Study* (1941), which has been so successful. Whereas the book by Frederick, Kitchen and McElwee is primarily a general treatise or informational text on how to study, Robinson's book is chiefly a manual or work book which contains in addition to the general theoretical material numerous detailed tests and exercises which must be worked out by the student. The book provides diagnostic tests of reading, tests of English usage, a study habits questionnaire, a handwriting analysis chart, a check list for the analysis of the student's notebook, and many other useful diagnostic instruments. Answer keys and norms for the various tests are included. This makes it possible for the student to discover his own particular weaknesses before attempting the necessary remedial work which is outlined in the manual. The organization and content of the book clearly indicates that the author believes that students learn to study effectively by practicing newer and better methods, rather than by merely learning the rules of how to study.

The book is carefully documented, interestingly written, and splendidly geared to the study needs of college students.

The two books are both outstanding contributions to the psychology

of study and should help greatly in ameliorating the condition described in the opening paragraph of this review.

GLENN MYERS BLAIR.

University of Illinois.

PEATMAN, JOHN GRAY. *Descriptive and sampling statistics.* New York: Harpers, 1947. Pp. xviii+577.

The outstanding virtue of this book is the profuse use of interesting illustrative data, presented not in a perfunctory manner, but with genuine concern for the real problems of research. It it is possible to communicate a feeling for the relationship of statistics to data, such a book as this will do it. Mastery of formal statistics without such a feeling is a poor asset to a psychologist.

The table of contents is much more detailed than usual. Glossary of symbols, glossary of formulas, and a considerable number of tables add to the usefulness of the book; however, the inclusion of four separate tables of areas under the normal curve seems excessive.

Finding errors in the book is not difficult. On p. 259 biserial r is erroneously stated not to assume a normal distribution for the continuous variable. A scattergram on p. 203 illustrating non-linear correlation is more notable for the fact that the lines that are supposed to go through the means of the two variables do not. Computational use of a table with an open-end interval, on p. 154 and elsewhere, is misleading. The same illustration is unfortunate for another reason, namely, that the mean comes out even, giving the student an erroneous impression of the amount of computational labor ordinarily involved in subsequent operations.

Typographical errors occur in formulas on pp. 165, 219, 418, 447, and 560. The most serious of these is an error in the formula for the standard deviation on p. 165, repeated on p. 560, since it coincides with a common student error, using coded data in part of the formula and decoded or deviate data in another part. The same error is encouraged by an awkward choice of notation in some of the formulas for correlation, as on p. 238.

A number of terms, including "normal curve" and "central tendency," are used more as in everyday speech than as in mathematical statistics. The terms "variable" and "representative sample" are not consistently defined. Categorical data are called "non-variable," and whether ranked data and discrete measured data are included in the term "variable" along with continuous measured data depends on which page you are reading. In chapter 11 the term "representative sample" is used repeatedly to mean an unbiased sample and also to mean a sample such that the statistic being studied has zero sampling error. Obviously, the whole study of sampling errors is based on a distinction of these two meanings. "Chance errors" are characterized as "just as

likely to affect the results positively as negatively" on p. 286 and as "just as likely to occur as not to occur" on p. 332. Neither statement is exactly correct.

In drawing frequency polygons the convention of bringing the graph down to the x-axis on each end is ignored. The idea that area represents frequency is thus obscured, leading to difficulty in explaining the relation of frequency polygons to probability curves. Indeed, the ordinate of probability curves is labelled "frequencies." The "principle of equiprobability" invoked in the discussion of the binomial distribution and the normal curve is not needed in deriving the normal curve as a limiting function of the binomial.

The book, however, is not characterized so much by an excess of errors as by a style at worst inexact, at best diffuse and repetitious. It must be admitted that to more students than not, this characteristic may seem more a virtue than a defect.

Certainly this book is a valuable addition to the library of any teacher of statistics. It seems adequate as a text of descriptive statistics; treatment of categorical data is outstandingly good. As a text of sampling statistics it is deficient, especially the chapter on probability and statistical inference. Nowhere, for example, are there definitions of "random variable," "mathematical expectation," or "biased statistic." The two types of errors in testing statistical hypotheses and the basis for selecting best statistical tests are not mentioned directly. The book is made vulnerable by being so inclusive as to serve as a reference manual; for example, specialists will certainly object to the chapter on factor analysis.

There is some danger that this book will be adopted by instructors in statistics who are themselves long on interest in data and short on precise knowledge of the logic of probability and statistical inference, and an equal danger that it will be ignored by those who are rigorous in notation and definition but do not integrate statistics with the content of research. Without urging that instructors adopt the text that they like least, one may at least admit that no elementary statistics text now available compensates for all possible deficiencies in the instructor's presentation.

JANE LOEVINGER.

St. Louis, Missouri.

BOOKS AND MATERIALS RECEIVED

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BOWLBY, JOHN. *Forty-four juvenile thieves: their characters and home-life.* London: Bailliere, Tindall & Cox, 1946. Pp. 56. 7s 6d.

BOWLER, EARL M., AND DAWSON, FRANCES TRIGG. *Counseling employees.* New York: Prentice-Hall, 1948. Pp. xi+236. $4.00.

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BRENNAN, ROBERT E. *The image of his maker.* Milwaukee: Bruce Publishing Co., 1948. Pp. ix+328. $3.25.

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